IPCC Working Group I Fourth Assessment Report

Expert and Government Review Comments on the Second-Order Draft

Chapter 9

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Please note that under IPCC procedures authors are required to take account of all substantive review comments in both review rounds. Thus responses to individual comments may be influenced by comments from other reviewers.

Batch AB (15 June 2006)

No.	Batch	Page:line From	Page:line To	Comment	Notes
9-1	A	0:0	0:0	It would be useful for chapter 2 to provide the probability for a net RF to be larger than 0.x Wm-2 (x being whatever chapter 9 needs to attribute confidently observed warming to human activities). [Olivier Boucher (Reviewer's comment ID #: 27-34)]	Accountted for. Chapter 2 now gives this probability, and it is cross-referenced in chapter 9.
9-2	A	0:0		Chapter 9 SOD has improved considerably and is very readable and informative [Fons Baede (Reviewer's comment ID #: 9-1)]	Thank you.
9-3	A	0:0		The Roesch (2006) paper (see references in Ch. 8) reported that all AR4 models had a positive albedo bias. I quote: "The mean annual surface albedo of the 15 AR4 models amounts to 0.140 with a standard deviation of 0.013. All AR4 models are slightly above the mean of PINKER (0.124) and ISCCP-FD (0.121)." This average albedo error in the models of 0.016 vs the PINKER data and 0.019 vs the ISCCP-FD, diminishes solar forcing on the order of 20 watts/m^2 or more in the plane orthogonal to the Sun. Since the models have demonstrated a good fit to the 20th century data, they must have compensating errors, most probably in their climate sensitivity to greenhouse gas forcing, since this varies so much between the models and is out of line with the climate sensitivity calculated from paleo-climate data. Solanki. et al, (2004) report that solar activity for the last 65 years has been at its highest level in 8000 years. Raimund Muscheler, et al (2005) confirm the current high level of solar activity but note that two other historical periods may be comparable. Separate climate commitment studies by Meehl, et al (2005) and Wigley, et al, (2005) report that temperature may take a century to equilibrate to new levels of forcing and over a thousand years for the climate to stablize. Proper relative attribution based on models obviously requires that their albedo errors first be corrected, and then climate commitment studies performed starting from the beginning of the time period attribution is being studied for. With the unusually high and constant level of solar activity over the latter half of the 20th century, a larger portion of the warming must be committed than has been previously reported in the studies referenced in the TAR and in this draft. The relatively constant level of solar activity over this period combined with the filtering of high frequency signals by ocean thermal inertia and systematic bias of the models, probably explains why the detection studies in this chapter did not detect a stronger solar signal. When these errors are corrected, I suspect the natural forcing (solar plus volcanic) will be a net contributer to the last 50 years of warming, although anthropogenic greenhouse gasses may still account for a majority of the warming. However, the decreased climate sensitivity to CO2 should moderate predicted future warming. S.K. Solanki, I.G. Usoskin, B. Kromer, M. Schussler, J. Beer (2004). "Unusual activity of the Sun during recent decades compared to the previous 11,000 years.". Nature 431: 1084-1087. DOI:10.1038/nature02995 Raimund Muscheler, Fortunat Joos, Simon	Rejected. Systematic biases in albedo would to first order not affect the detection and attribution results (see 9.2.3). Apart from that, it is not clear if the difference between models and observations is really significant. There is also no evidence that climate sensitivity inferred from models is inconsistent with that inferred from paleo-observations (see sections 9.3 and 9.6).

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				A. Müller and Ian Snowball (2005). "Climate: How unusual is today's solar activity?". Nature 436: E3-E4. DOI:10.1038/nature04045 S. K. Solanki, I. G. Usoskin, B. Kromer, M. Schüssler and J. Beer (2005). "Climate: How unusual is today's solar activity? (Reply)". Nature 436: E4-E5. DOI:10.1038/nature04046 Meehl G. A., et al. Sciencexpress, 10.1126/science.1106663 (2005). Wigley T. M. L., et al. Sciencexpress, 110.1126/science.1103934 (2005). [Martin Lewitt (Reviewer's comment ID #: 146-4)]	
9-4	A	0:0		Assuming working group 2 will still have its chapter 19 on observed changes, it would be appropriate that this chapter and that chapter reference each other appropriately [Michael Manton (Reviewer's comment ID #: 157-35)]	Rejected. Chapter I WG2 reports on impacts related to observed changes in the physical system, therefore the connection is mostly one-way. The issue of overlap in the WG1/WG2 assessments of change in the physical climate system has been brought to the attention of the WG1 and WG2 co-chairs for resolution.
9-5	A	0:0		This is a very nice chapter. Besides a few small technical comments, I have one overall comment regarding the coordination with Chapter 4. Most of the discussion of land ice in Chapt. 9 is simply referencing Chapt. 4. But in Chapter 4, the discussion of formal attribution is scant. This is likely due to inherent challenges or lack of studies, but it feels like some comments in chapter 9 would be appropriate. This is not my area of expertise, so I do not offer specific comments, simply note the perceived gap. [J. David Neelin (Reviewer's comment ID #: 187-17)]	Accounted for, the reviewer is correct that studies are few, and those we are aware of are referenced. We do have a section on land ice changes, and an overall assessment that is mentioned in the Executive summary, since we agree this is an important topic.
9-6	A	0:0		This chapter has a tendency to list various studies without providing a clear synthesis or overall assessment of the issues. The approach appears in many of the paragraphs. It is very important that this attribution chapter provide this expert analysis, using the IPCC statistical meanings of likely, very likely, etc. If no such judgment is possible, then that should clearly be stated. Examples are given below, but an overall review of the chapter should be conducted with this in mind. [Govt. of United States of America (Reviewer's comment ID #: 2023-542)]	Taken into account where feasible. Likelihood statements are given as far as possible, particularly in the summary sections. We also draw the reviewers attention to Section 9.7 and Table 9.7.1 which reviews the extensive list of assessments using calibrated language that have been made in the Chapter.
9-7	A	0:0		In the figures, there is an uneven distribution of how many models are used for each point. It is awkward, and raises questions of why certain models were picked to illustrate which point. Furthermore, comparisons of the results between the different figures is impossible under these circumstances. [Govt. of United States of America (Reviewer's comment ID #: 2023-543)]	There are several reasons why the number of models used varies between figures. The selection of models is beyond the control of the authors of this chapter in the case of figures reproduced directly from the literature.

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					In the case of figures produced explicitly for this chapter, the selection of models was subject to criteria such as whether the model was available at the time of production of the diagram, whether the particular variable of interest was available from a given model, and whether the model satisfied a basic drift metric. Where possible, model selection criteria are now stated either in the caption, the text, or in the supplementary information.
9-8	A	0:0		Here, and through the IPCC report in general, the ± values should be noted as implying one standard deviation, or 'likely'. [Govt. of United States of America (Reviewer's comment ID #: 2023-544)]	The interpretation of uncertainty bands has been reviewed and clarified where necessary.
9-9	A	0:0		External, internal, natural and anthropogenic are used in confusing manners. Simplification would be better. The primary distinction should be between anthropogenic and non-anthropogenic. To say 'external anthropogenic' forcing is confusing, since the anthropogenic forcing is internal to the troposphere. [Govt. of United States of America (Reviewer's comment ID #: 2023-545)]	Rejected. We feel that our use of these terms is transparent. In order to understand, as fully as possible, the recent history of the climate system, it is necessary to clearly distinguish between variability that results from anthropogenic forcing (an external influence on the climate system), natural external forcing (such as solar and volcanic forcing) and that which results from processes entirely internal to the climate system (see Section 9.1)
9-10	A	0:0		Consider whether the summary for each subsection should be in the front of each subsection (like the Executive Summary is in the front of the chapter). [Govt. of United States of America (Reviewer's comment ID #: 2023-546)]	Rejected. We prefer to present the evidence, synthesize in our summaries, and present a single executive summary.This style is largely used within the report.
9-11	A	0:0		This chapter is interesting, because it clearly presents all the factors being able to explain the warming current and future one of the ground leading to the climatic change. From my new reading of this chapter, it comes out that my remarks its taken into account. The figures are improved of same as some results. The illisibles figures are removed. Nuances are brought to avoid easy assertions.	Noted, thank you.

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				Sight the current results, this chapter can be validated. Congratulation with the members of group I [Expédit Wilfrid VISSIN (Reviewer's comment ID #: 277-1)]	
9-1158	B	0:0		This is a comprehensive description of the difficult problem of attribution of changes in climate to a range of possible causes. It is difficult to reach certain conclusions, but the report describes a careful, systematic process to arrive at estimates of the probability that various changes are due to anthropogenic influences. We believe that the conclusions reached are the best possible at the current state of knowledge. The conclusions are stated in a moderate manner, with due attention to the inevitable uncertainties. The range of literature surveyed is encyclopedic.The writing of the report is clear and unambiguous, if also somewhat prolix. [Govt. of Ireland (Reviewer's comment ID #: 2025-4)]	Thank you. In revising the chapter we have attempted to tighten up some passages in an attempt to be less prolix
9-12	A	1:9	1:9	Can cheekly I ask that my second initial be included - "G.S. Jones". My name isn't that unique, even amongst the climate science community. [Gareth S. Jones (Reviewer's comment ID #: 121-75)]	Accounted for. This question has been referred to the TSU because resolution would require an adjustment to the Endnote style file that is used by WG1.
9-13	A	3:1		Executive Summary: The inverse calculations give much stronger constraints on total aerosol forcing than the forward calculations, and they also have a better foundation than the forward calculations because they are based on actual observed temperature changes. I recommend that a paragraph be added to the Executive Summary summarizing the constraints on the total aerosol forcing given in section 9.2.1.2 and table 9.2.1 [Peter Stone (Reviewer's comment ID #: 257-3)]	Rejected. At this point, the inverse aerosol estimates are quite consistent with those given in chapter 2, as now detailed in the 9.2 text and summary. We have not propagated this into the summary, since we wanted to focus on the most important findings
9-14	A	3:3	3:3	Delete from "Evidence" on line 3 to "TAR" on line 4" This sentence is arrant nonsense. "The climate" is a complex "system" involving a large number of "influences". But how can these be divided into "external" and "internal". The El Niño ocean anomaly takes place on earth, so you call it "internal"; yet it has a major influence on the lower atmosphere, so it is "external as well. Volcanoes may be considered "external", but they influence the climate on the surface directly. Thi division is misleading and unneceaasry [VINCENT GRAY (Reviewer's comment ID #: 88-902)]	Rejected. The separation between internal and external causes of climate variations is standard practice.
9-15	A	3:3		Editorial: I suggest that paragraphs in the Exec Summary should be short and each quite narrowly focused. Several in this draft are long and a bit rambling because of that. Also this first paragraph seems to want to say everything which then leads to repetition further down. [Martin Manning (Reviewer's comment ID #: 155-55)]	Accepted. We have reviewed and revised our executive summary with a view to avoiding repetition and being as concise as possible.We have also split paragraphs where useful.
9-16	A	3:4	3:4	Delete "widespread"	Rejected. The reviewer provides no

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				[VINCENT GRAY (Reviewer's comment ID #: 88-903)]	supporting reasoning for the proposed change.
9-17	A	3:4	3:4	Replace "anthropogenic warming of" with "human influence on" [VINCENT GRAY (Reviewer's comment ID #: 88-904)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-18	A	3:4	3:4	Delete "system" [VINCENT GRAY (Reviewer's comment ID #: 88-905)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-19	A	3:4	3:4	Replace "in" by "from" [VINCENT GRAY (Reviewer's comment ID #: 88-906)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-20	A	3:5	3:5	Insert after "surface" "due to the proximity of measuring equipment to human activity. However, measurements" [VINCENT GRAY (Reviewer's comment ID #: 88-907)]	Rejected. The reviewer provides no supporting reasoning for the proposed change. The suggested insertion is incorrect.
9-21	A	3:5	3:5	Insert after "oceans" "do not show obvious human influence". [VINCENT GRAY (Reviewer's comment ID #: 88-908)]	Rejected. The reviewer provides no supporting reasoning for the proposed change. The proposed insertion is incorrect.
9-22	A	3:5	3:5	Replace "very likely" by "most unlikely" [VINCENT GRAY (Reviewer's comment ID #: 88-909)]	Rejected. The reviewer provides no supporting reasoning for the proposed change. There is no evidence to support the proposed change.
9-23	A	3:5	3:5	Change "atmospheric" to "atmosphere". [David Parker (Reviewer's comment ID #: 195-123)]	Accepted.
9-24	A	3:5	3:8	I find that it is "very likely that greenhouse gas forcing has been the dominant cause" difficult to reconcile with "it is highly likely that warming... cannot be explained without external forcing". [Daithi Stone (Reviewer's comment ID #: 256-1)]	Rejected. The second statement is less specific so should have a higher confidence associated with it.
9-25	A	3:5		.. in the free atmosphere and in the oceans. [Govt. of Austria (Reviewer's comment ID #: 2002-47)]	Accepted. See 9-23.
9-26	A	3:6	3:6	Insert after "warming" "on the surface" [VINCENT GRAY (Reviewer's comment ID #: 88-910)]	Rejected. The reviewer provides no supporting reasoning for the proposed change. Warming has also been observed in the oceans and free atmosphere.

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9-27	A	3:6	3:18	Delete from "and it is" on line 6 to "century" on line 18. There is simply no evidence for these extravagant assertions [VINCENT GRAY (Reviewer's comment ID #: 88-911)]	Rejected. The reviewer provides no supporting reasoning for the proposed change. A brief overview of the evidence is provided in the following >100 pages.
9-28	A	3:8		Some of the greenhouse gas warming was likely offset by other factors (e.g., aerosols). [Govt. of United States of America (Reviewer's comment ID #: 2023-547)]	Accepted. Aerosols now mentioned.
9-29	A	3:10	3:11	The basis for saying that the temperature changes of the past half century took place at a time when natural focing would be expected to have produced cooling is far from clear. Section 2.7 (Pg 2-53, lines 39-53) indicates no trend in the past 25 years in solar irradiance. The section also discusses two major volcanic eruptions (El Chichon in 1982 and Pinatubo in 1991), but then concludes that stratospheric aerosol concentrations are at their lowest concentrations since the satellite era and global coverage began in the late 1970s (Pg 2-58, lines 55-56). These facts would seem to indicate no effect of solar variability and a declining effect of volcanic activity for that period. [Lenny Bernstein (Reviewer's comment ID #: 20-66)]	Taken into account. Text in body of chapter has been clarified to explain that simulations of the 20th century using natural forcing only produce a cooling.
9-30	A	3:10	3:10	Amend text to "The warming in both oceans and atmosphere took place…" as line 3 already states that there has been warming in both atmosphere and oceans. [David Parker (Reviewer's comment ID #: 195-124)]	Accepted. Text revised.
9-31	A	3:10	:11	The basis for saying that the temperature changes of the past half century took place at a time when natural forcing would be expected to have produced cooling is far from clear. Section 2.7 (Pg 2-53, lines 39-53) indicates no trend in the past 25 years in solar irradiance. The section also discusses two major volcanic eruptions (El Chichon in 1982 and Pinatubo in 1991), but then concludes that stratospheric aerosol concentrations are at their lowest concentrations since the satellite era and global coverage began in the late 1970s (Pg 2-58, lines 55-56). These facts would seem to indicate no effect of solar variability and a declining effect of volcanic activity for that period. A greater exposition should be given about what 'likely' truly means in this context, and whether these numbers can be truly quantified over the last 50 years given the uncertainties in both solar and volcanic aerosol reconstructions. [Govt. of United States of America (Reviewer's comment ID #: 2023-548)]	See 9-29 response. Likely is based on the IPCC likelihood terminology and is in our view consistent with the uncertainties.
9-32	A	3:10	:11	Are natural external causes and non-anthropogenic external forces the same thing? Why use different terminology? [Govt. of United States of America (Reviewer's comment ID #: 2023-549)]	Accepted. "non-anthropogenic" replaced with "natural".
9-33	A	3:17	3:18	Much of the more certain evidence presented in this chapter concerns the attribution of changes in temperature. Hence the conclusion that "…anthropogenic forcing has caused substantial change in the climate system…" is a little strong. Perhaps "the temperature	Rejected. We find the restriction to the thermodynamic components of the system too narrow here.

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				components of" or "thermodynamic components of" could be added before "climate system". [Matthew Collins (Reviewer's comment ID #: 44-1)]	
9-34	A	3:20	3:21	Delete from ."of the climate system"" on line 20 to "system" on line 21 [VINCENT GRAY (Reviewer's comment ID #: 88-912)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-35	A	3:21	3:21	Insert after "surface" "because of the proximity of measuring equipment to human activity. However, measurements" [VINCENT GRAY (Reviewer's comment ID #: 88-913)]	Rejected. The reviewer provides no supporting reasoning for the proposed change. The proposed change is incorrect.
9-36	A	3:21		"Anthropogenic warming ..." can only be detected in the lower atmosphere (troposphere). The term "free atmosphere" is used in chapter 3 and the remainder of chapter 9 as meaning all of the atmosphere above the earth/atmosphere boundary layer, i.e. both troposphere and stratosphere. [Govt. of Germany (Reviewer's comment ID #: 2011-129)]	Accounted for, sentence has been revised.
9-37	A	3:22	3:22	Insert after "oceans" ."do not show evidence of human influence" [VINCENT GRAY (Reviewer's comment ID #: 88-914)]	Rejected. The reviewer provides no supporting reasoning for the proposed change. The proposed change is incorrect.
9-38	A	3:28	3:28	Add at beginning "the complete absence of" [VINCENT GRAY (Reviewer's comment ID #: 88-915)]	Rejected. The reviewer provides no supporting reasoning for the proposed change. The proposed change is incorrect.
9-39	A	3:28	3:29	The reference to "widespread radiosonde measurements" having "started in the late 1950s" is justifiable in a sense, but probably rather misleading. A widespread radiosonde network for the northern hemisphere evolved in the late 1940s, in particular with the establishment of the ocean weather ships in the North Atlantic and North Pacific. Coverage was boosted later in the 1950s when the sparse set of radiosonde stations in the southern hemisphere was enhanced in 1957 for the IGY. But it could be argued (based on reanalysis results) that the network in the southern hemisphere was still not adequate until enhanced by comprehensive satellite, drifting buoy and aircraft measurements in the late 1970s, despite some improvements prior to that, notably the availability of VTPR satellite data late in 1972. In summary, an adequate global observing system for the free atmosphere was not in place until the late 1970s, whereas for assessment of free-atmosphere changes in the northern hemisphere, one might be able to go back beyond the late 1950s. Perhaps the sentence could end "since the establishment of the radiosonde observing network in the 1940s and 50s" with the possible addition of ", and in particular	Accepted. This sentence has been rephrased so that it avoids making reference to the history of development of the radiosonde network.

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				since the enhance ment of the observing system with the introduction of satellite and other new types of observation in the 1970s". [Adrian Simmons (Reviewer's comment ID #: 242-126)]	
9-40	A	3:29	3:30	Replace "and stratospheric cooling is very likely" with "cannot possibly be" [VINCENT GRAY (Reviewer's comment ID #: 88-916)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-41	A	3:29	3:29	Replace "and" with "but there may have been an influence on" [VINCENT GRAY (Reviewer's comment ID #: 88-917)]	Rejected. The reviewer provides no supporting reasoning for the proposed change. The proposed wording change does not make sense.
9-42	A	3:31	3:32	Replace "a warming" with "very recent warming in the" [VINCENT GRAY (Reviewer's comment ID #: 88-918)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-43	A	3:32	3:32	Replace "likely led to" by "possibly been a factor in" [VINCENT GRAY (Reviewer's comment ID #: 88-919)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-44	A	3:32	3:32	Replace "It is likely" with "It is improbable" [VINCENT GRAY (Reviewer's comment ID #: 88-920)]	Rejected. The reviewer provides no supporting reasoning for the proposed change. The proposed wording change runs counter to all the evidence.
9-45	A	3:33	3:33	Insert after "observed". "periodic" [VINCENT GRAY (Reviewer's comment ID #: 88-921)]	Rejected. The reviewer provides no supporting reasoning for the proposed change. There is no evidence supporting the proposed change.
9-46	A	3:33	3:33	Add "general" after observed and before warming. It is not warming everywhere. [Ronald J Stouffer (Reviewer's comment ID #: 258-12)]	Accepted. Text revised.
9-47	A	3:34	3:34	Delete "Anthropogenic forcing, resulting in" [VINCENT GRAY (Reviewer's comment ID #: 88-922)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-48	A	3:35	3:35	Replace "likely.the largest" by "a" [VINCENT GRAY (Reviewer's comment ID #: 88-923)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-49	A	3:37	3:41	Delete this whole paragraph, for which there is no credible evidence. There is, however, evidence, that where there is a thorough correction of weather station information, as has been carried out in the continetal USA , and in China, the supposed "warming" all but disappears	Rejected. The observations have been adjusted for changes in exposure and instrumentation. In countries (eg Australia) where this has been carried

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				[VINCENT GRAY (Reviewer's comment ID #: 88-924)]	out with great care, warming is still evident.
9-50	A	3:37	3:38	This statement might be fine in the chapter, but the Exec Summary is going to be read by some people who will assume that you are implying a connection between habitation of a continent and its temperature change. So here at least I think you need to say "all of the continents except Antarctica". [Martin Manning (Reviewer's comment ID #: 155-56)]	Accepted. Text revised.
9-51	A	3:43	3:43	Statements of the form "evidence is emerging" presumably combine information from observations but also from simulations of future climate change from models and/or information about changes which are expected on the grounds of simple physical reasoning e.g. the reduction of the number of frost days. Although reasonable to me, I fear they will be open to critism as the may be interpreted as saying "we don't have evidence yet but will find it pretty darn soon". Just a word caution really. Perhaps it would be better to say "Anthropogenic influence on surface temperature extreme temperatures is consistent with our understanding of climate change", thus linking with the title of the chapter. [Matthew Collins (Reviewer's comment ID #: 44-2)]	Taken into account. We have scrutinized all statements on emerging evidence in the chapter and have rephrased virtually all of them.
9-52	A	3:46	3:48	Replace from "and there is evidence" on line 46 to "heat wave" on line 48 with "due mainly , to greater heating in cities, particularly at night, and in the winter". [VINCENT GRAY (Reviewer's comment ID #: 88-925)]	Rejected. The reviewer provides no supporting reasoning for the proposed change. The warming is not more evident in cities than in rural areas and oceans.
9-53	A	3:47	3:48	The notion of the attribution of the risk of a particular event like the 2003 heatwave is a new development since the TAR and perhaps this should be pointed out. I suspect we will see more such studies in the future. [Matthew Collins (Reviewer's comment ID #: 44-3)]	Rejected. We don't think the ES is the place to list methological developments.
9-54	A	3:48	3:48	One "heat wave" does not make a "trend" [VINCENT GRAY (Reviewer's comment ID #: 88-926)]	Rejected. The European heat wave is just a single example and this is clear in the current text.
9-55	A	3:50	3:51	Delete from ".Anthropogenic" in line 50 to "recent" in line 51. There is no evidence that these changes are "anthropogenic" [VINCENT GRAY (Reviewer's comment ID #: 88-927)]	Rejected. See page 51 for brief overview of evidence.
9-56	A	3:51	3:52	"global trend in snow cover" might be better than "trend in global snow cover" which suggests a global blanket of snow to me. [Matthew Collins (Reviewer's comment ID #: 44-4)]	Accepted. Text now refers to trend in snow cover extent (analogous to the commonly used term sea-ice extent).
9-57	A	3:51	3:51	Delete "There is evidence of" [VINCENT GRAY (Reviewer's comment ID #: 88-928)]	Rejected. The reviewer provides no supporting reasoning for the proposed

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					change.
9-58	A	3:52	3:52	Insert before "consistent". "are" [VINCENT GRAY (Reviewer's comment ID #: 88-929)]	Rejected. The proposed change would make the sentence ungrammatical.
9-59	A	3:52	3:52	"replace "consistent witt" by "possibly influenced by"" [VINCENT GRAY (Reviewer's comment ID #: 88-930)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-60	A	3:52	3:52	Insert before "warming". "very recent" [VINCENT GRAY (Reviewer's comment ID #: 88-931)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-61	A	3:52	3:52	Delete "evidence that" [VINCENT GRAY (Reviewer's comment ID #: 88-932)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-62	A	3:53	3:53	Replace "has also likely" by "may have" [VINCENT GRAY (Reviewer's comment ID #: 88-933)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-63	A	3:55	3:57	It think it is too strong to say the trends in the NAM are likely to be related in part to human activity. Trends are larger than seen in climate model control situations and some models project a weak change under a stronger forcing than experienced in the historical period (i.e. in future runs - dealt with by chapter 10).It is safter to conclude that trends in the NAM are unlikely to be due wholey to natural causes rather than the stronger attribution statement. [Matthew Collins (Reviewer's comment ID #: 44-5)]	Rejected. The proposed wording is equivalent to our present wording, and the following sentence draws attention to the inconsistency in amplitude.
9-64	A	3:55	9:55	Add at beginning "There have been" [VINCENT GRAY (Reviewer's comment ID #: 88-934)]	Rejected. Simple issue of style.
9-65	A	3:55	:56	"likely related in part" is less than transparent language. Reword. [Govt. of United States of America (Reviewer's comment ID #: 2023-550)]	Rejected. The following sentence explains why only a part of the change may be of anthropogenic origin.
9-66	A	3:56	3:56	Delete "are likely related in part to human activity" There is no evidence for this statement. Models alone are not enough [VINCENT GRAY (Reviewer's comment ID #: 88-935)]	Rejected. Assessment is based on evidence from observations using models, data and physical reasoning.
9-67	A	3:57	3:57	The NAO may be some regional manifestation of the NAM or an independent "mode". Whatever, it is a commonly used term and is discussed, for example, extensively in WG2.It should be mentioned here. [Matthew Collins (Reviewer's comment ID #: 44-6)]	Rejected. Distinction between the NAM and the NAO is discussed extensively in chapter 3. No study assessed on NAO attributable changes
9-68	A	4:2	4:2	Delete from "leading" to "pressure". There is no evidence for this statement.. Models are not enough and it is probably natural variability	Rejected. The reviewer provides no evidence to support his assertion.

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				[VINCENT GRAY (Reviewer's comment ID #: 88-936)]	
9-69	A	4:4	4:12	This paragraph is rather weak in its conclusions with phrases such as "suggestive of a possible human influence". I wonder if it should really appear in the executive summary. [Matthew Collins (Reviewer's comment ID #: 44-7)]	Partly accepted. Text has been revised, but we do feel that discussion of uncertain, but important, results improves balance. Note that heading of this paragraph has also changed to improve balance.
9-70	A	4:5	4:6	Presumably in this sentence the comma should be removed after "Observed"; and "with" should appear on line 6 between consistent and simulations? [Martin Manning (Reviewer's comment ID #: 155-57)]	Agreed. Text revised.
9-71	A	4:6	4:6	Insert after "consistent" "with" [VINCENT GRAY (Reviewer's comment ID #: 88-937)]	Accepted.
9-72	A	4:6	4:6	Insert in front of "suggestive" "vaguely" [VINCENT GRAY (Reviewer's comment ID #: 88-938)]	Rejected. The sentence is already very weak, correctly representing the status of the evidence.
9-73	A	4:6	4:6	Insert "with" before "simulations" [David Parker (Reviewer's comment ID #: 195-125)]	Agreed.
9-74	A	4:6		..appear to be qualitatively consistent with simulations of the .. [Govt. of Austria (Reviewer's comment ID #: 2002-48)]	Agreed.
9-75	A	4:6		Replace ""simulations of the 20th century" by "with model simulations" [Fons Baede (Reviewer's comment ID #: 9-2)]	Partly accepted. We do not simulate models. "With" added, and text has been revised.
9-76	A	4:7	4:9	The more I read this sentence the more I find it self contradictory. Can you clarify. [Martin Manning (Reviewer's comment ID #: 155-58)]	Accepted, sentence revised
9-77	A	4:9	4:11	It is proposed to improve the clarity of the sentence begiing with: The observed increase …. The sentence might read as follows: The observed increase in the proportion of very intense hurricanes is the same direction as suggested by theoretical studies and modelling studies of projected 21st century change. However, observation and models do not agree with regard to the magnitude of the change. [Govt. of Austria (Reviewer's comment ID #: 2002-49)]	Accepted. Text revised.
9-78	A	4:9	9:11	This sentence does not read well. Please repair or improve. [Fons Baede (Reviewer's comment ID #: 9-3)]	Accepted. See 9-77
9-79	A	4:9	:12	The last sentence in particular sounds a little awkward. It seems to me that we have enough understanding of the process of storm formation to conclude that hurricane or cyclone intensity is correlated to SST. This should suffice as a statement on attributability. [Govt. of Germany (Reviewer's comment ID #: 2011-130)]	Sentence revised. Current understanding is in our view insufficient for confidently attributing changes in hurricane activity to SST

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					changes
9-80	A	4:9	:12	Sentences beginning “The observed….” , and ending “…factors”, are vague. Rewrite and remove last sentence. [Govt. of United States of America (Reviewer’s comment ID #: 2023-551)]	Disagree with deletion of last sentence – we find it necessary to point out problems that lead to overall assessment of hurricane changes.
9-81	A	4:10	4:13	There is some dispute over the observational data used in the studies that show observed inceases in intense hurricanes (e.g. Landsea Nature 438 E11 2005). [Ruth McDonald (Reviewer’s comment ID #: 173-29)]	Agreed – that is why we point out monitoring problems.
9-82	A	4:11	4:11	Amend text to "Inadequacies in knowledge of processes, in understanding of natural variability, in modelling, and in monitoring…" [David Parker (Reviewer’s comment ID #: 195-126)]	Accepted. Text revised.
9-83	A	4:12	4:12	Add "in the past" after "tropical cyclones"? [Ronald J Stouffer (Reviewer’s comment ID #: 258-13)]	Accepted. Text revised.
9-84	A	4:15	4:15	Insert after "climate" "but they have not been particularly successful" [VINCENT GRAY (Reviewer’s comment ID #: 88-939)]	Rejected. The reviewer provides no supporting reasoning for the proposed change. The proposed addition is untrue.
9-85	A	4:17	4:17	the sentence is too vague and the word successful may need to be mitigated a little. [Govt. of France (Reviewer’s comment ID #: 2010-56)]	Sentence revised
9-86	A	4:17	5:17	I have not been asked to correct the Gloasary, but it has to be changed to help clear up the confusion. "Climate" may be defined as "the totality of all the external and internal influences on the surface of the earth and its neighbouring atmosphere:, usually applied to influences longer tha the hourly, daily, and weekly influences which are called 'weather'" [VINCENT GRAY (Reviewer’s comment ID #: 88-957)]	Rejected. The definition of climate is standard and sensible. There is no confusion here.
9-87	A	4:18	:19	“A large fraction…..” Be more specific. Don't know natural variability or solar variability, so 'a large fraction' and 'very likely' seem too strong. Better to say it was very likely not the result of anthropogenic forcing [Govt. of United States of America (Reviewer’s comment ID #: 2023-552)]	Rejected. While uncertainties in paleoclimatic reconstructions and forcing reconstructions are substantial, both are virtually independent, so finding a correspondence by change is unlikely. Text in chapter and overall results table has been revised to clarify. However, based on few studies and active area of research, we did not feel comfortable giving a number for the variance explained, however, estimates are given in the chapter body.

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9-88	A	4:20	4:20	Replace "natural external forcing" by "changes in atmospheric composition". The phrase "natural extrenal forcing" is meaningless. Is this wat you actually mean by it? [VINCENT GRAY (Reviewer's comment ID #: 88-940)]	Rejected. "Natural external forcing" also includes solar variations and volcanic causes. This is explained carefully in the chapter and is standard terminology.
9-89	A	4:20	4:21	Delete from "Furhermore" on line 20 to "forcing" on line 21. Ther is no supporting evidence for this statement [VINCENT GRAY (Reviewer's comment ID #: 88-941)]	Rejected. The supporting evidence is briefly described throughout this chapter.
9-90	A	4:20	4:21	Ex. Sum.: "Furthermore, the 20th century warming evident in these records, *most of which end by the mid-20th century*, is likely due in part to anthropogenic forcing." Most of the records end by mid-20th century? Why? [Melinda Marquis (Reviewer's comment ID #: 162-80)]	The piece of text identified by the reviewer has been deleted to clarify the text (no need to descrbe the records in the ES).
9-91	A	4:21		I think you mean "natural" here not "anthropogenic" (?) or I can't follow the argument. [Joanna Haigh (Reviewer's comment ID #: 95-9)]	Accounted for, sentence has been revised.
9-92	A	4:23	4:28	Reading the underlying section and the box in chapter 10, it is not clear what is the basis for raising the lower best estimate of climate sensitivity from 1.5 to 2.0. This conclusion is repeated many times in this report without a clear basis. Many of the studies referred to have significant probability densities between 1.5 and 2? Furthermore, 2 of the 7 model results that were the basis for the TAR estimates of future warming had a climate sensitivity below 2 (see TAR table 9.A1). Suggest explanation and revision of conclusion as appropriate, and suggest to focus discussion of climate senstivity in one place (Chapter 10) and not include in Chapter 9. [Haroon Kheshgi (Reviewer's comment ID #: 125-36)]	Text revised, and inferences from chapter 9 have now been clearly separted from overall inferences in ES. Suggestion to delete sensitivity section in ch9 rejected, the overall assessment in ch10 is clearly crossreferenced , and chapter 9 is the appropriate place for estimates based on observed climate change.
9-93	A	4:23	4:28	Chapter 10 (page 3, lines 48 – 53) makes essentially the same statement as this but gives a slightly different description of the provenance of the result. The issue here is not repetition, but whether or not the two chapters are using independent evidence that just happens to produce exactly the same values of the climate sensitivity range, or using a common pool of evidence. Because the latter is intended I think some cross-chapter referencing is required. [Martin Manning (Reviewer's comment ID #: 155-59)]	Overlap has been resolved, and conclusions drawn from estimates of climate sensitivity based on observed climate change are now separated from the overall conclusions. See 9-92.
9-94	A	4:25	4:26	I read the "likely 2 to 4.5C" statement as a statement that there is some fundamental uncertainty on sensitivity but we do not know that uncertainty with certainty (only "likely"). ie the uncertainty range is likely to be 2 to 4.5C. I think what you want to say though is that the 2 to 4.5C is a 90-99% confidence range, right? [Daithi Stone (Reviewer's comment ID #: 256-2)]	Rejected. The assessment is that it is likely (not very likely) that the ECS likes between 2C and 4.5C.
9-95	A	4:27	4:28	The transient climate response is better constrained *in absolute terms*. I think this qualifier should be added. The TCR may be more uncertain in relative terms as it is	Text revised to indicate that better constraint for the upper limit

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				determined by the same atmosphere/surface feedbacks that set the climate sensitivity and processes which remove heat from the surface to the deep ocean. [Matthew Collins (Reviewer's comment ID #: 44-8)]	
9-96	A	4:27	4:28	this is a strong statement… [David Rind (Reviewer's comment ID #: 214-87)]	Noted. Statement revised somewhat to reflect extensive cross-chapter discussions. The statement remains strong, however.
9-97	A	4:27	4:28	Delete? How does one constrain an idealized experiment? Much more is needed if kept. It does not seem worth it. [Ronald J Stouffer (Reviewer's comment ID #: 258-14)]	Accounted for. TCR is not more idealized than ECS. TCR is explained in the glossary and in 9.6, where its relevance is discussed. We thus don't think that more explanation is needed in the ES.
9-98	A	4:27	:28	This is a strong statement…this is not really constrained by observations. It is simply an extrapolation and a model result. [Govt. of United States of America (Reviewer's comment ID #: 2023-553)]	Rejected. There are observational constraints. See also 9-97. However, origin of overall constraint estimate from Box 10.2 is now more clearly referenced.
9-99	A	4:28	4:28	Insert after "to" "the utterly ridiculous assumption of" [VINCENT GRAY (Reviewer's comment ID #: 88-942)]	Rejected. The reviewer's proposed wording is unscientific.
9-100	A	4:30	4:30	Again I think "thermodynamic aspects of" could be added before climate. [Matthew Collins (Reviewer's comment ID #: 44-9)]	Rejected. See 9-33.
9-101	A	4:30	4:37	Delete this whole paragraph. It ius not suported by evidence. Only "internal variability" is considered, not the possibility of periodic change (for example for ocean warming and ice extent) and no atenntion to possible influence of long term trends associated with changes in the sun, or in ocean circulation [VINCENT GRAY (Reviewer's comment ID #: 88-943)]	Rejected. The paragraph is about how all the lines of evidence support each other and indicate that the observed changes are not likely due to internal variability.
9-102	A	4:30	:32	"…many observed changes….are distinct from internal variability". This sentence lacks specificity, and is too strong in tone. This is too strong a statement to be left this ambiguous - be specific, which changes in particular? And what uncertainties afflict them? Does internal variability include, in this context, natural forcing as well? [Govt. of United States of America (Reviewer's comment ID #: 2023-554)]	Text has been revised to clarify that this section pulls together evidence discussed above.
9-103	A	4:33	4:33	Insert "and" before "some". [David Parker (Reviewer's comment ID #: 195-127)]	Agreed.
9-104	A	4:33	:33	Word missing between 'extent' and 'some' [Steve Harangozo (Reviewer's comment ID #: 98-14)]	Agreed

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9-105	A	4:34	4:37	Is it really obvious that equipartition of an engery change implies it is externally forced? It probably implies only that it is large-scale. [Michael Manton (Reviewer's comment ID #: 157-31)]	Rejected. Statement provides our assessment that probability of all components simultaneously increasing heat is smaller than for individual components
9-106	A	4:39	4:57	Remaining uncertainites. It seems that it is wroth adding some discussion on attributions studies for smaller than global scale regions. As one goes to smaller space scales, the uniqueness of the signal becomes a larger and larger problem. Also it is not obvious to me that the models are good surrogates for the noise. Both of these issues hinder making regional statements. [Ronald J Stouffer (Reviewer's comment ID #: 258-15)]	Accepted. A paragraph explaining larger uncertainties for smaller scales has been added to the executive summary.
9-107	A	4:42	4:42	Change "However" to "Nevertheless". [David Parker (Reviewer's comment ID #: 195-128)]	Accepted.
9-108	A	4:45	4:45	correct ".." [Pascale DELECLUSE (Reviewer's comment ID #: 58-44)]	Agreed.
9-109	A	4:51	4:51	the sentence is not really clear as it is not possible to directly validate simulated internal climate variability (it can't be drawn from observations only, page 5, line 44-46). It may be better to say: "…confidence in climate model simulated internal and external natural variability." [Govt. of France (Reviewer's comment ID #: 2010-57)]	Accepted. Section has been revised to clarify.
9-110	A	4:51	4:51	Add at end "but there has been no confirmation from actual observatiomns" [VINCENT GRAY (Reviewer's comment ID #: 88-944)]	Rejected. The reviewer provides no supporting reasoning for the proposed change. There is substantial evidence that the models simulate past observed changes, and detection and attribution results are based on observed climate change.
9-111	A	4:53	4:53	Replace "some" by "most" [VINCENT GRAY (Reviewer's comment ID #: 88-945)]	Rejected. The majority of records used are adequate in spatial coverage and accuracy, for the purposes of detection and attribution.
9-112	A	4:54	4:54	Delete "While" [VINCENT GRAY (Reviewer's comment ID #: 88-946)]	Rejected. Change would make the sentence ungrammatical.
9-113	A	4:55	4:55	Replace "still affect confidence in estimates of the": with "now provide no evidence whatsoever of an" [VINCENT GRAY (Reviewer's comment ID #: 88-947)]	Rejected. Proposed changed wording is untrue.

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9-114	A	4:56	4:56	The phrase "ristrict D&A of changes in extremes" assumes that changes have happened but just can't be seen. Again, I am willing to accept this but it may be open to criticsm (see comment 2 above). [Matthew Collins (Reviewer's comment ID #: 44-10)]	Accepted. Wording has been revised.
9-115	A	5:4	5:4	Delete from "as expressions" to "change" This distinction is confusing. You are trying to find out whether human emissions of greenhouse gases have an effect on nthe climate and you should say so. This artificial distinction seems to have the object of covering up human influences on the earth's surface which do not involve greenhouse gas emissions, such as the building of cities, the use of energy, and changing land use. [VINCENT GRAY (Reviewer's comment ID #: 88-948)]	Rejected. The purpose of this chapter is to understand all climate changes, not just to attribute them to greenhouse gases. An important aspect, and one the chapter considers in detail, is how much of the recent changes could be explained by natural climate variability and internal processes.
9-116	A	5:4	5:6	There seems to be potential for confusion between this statement about adopting a broader scope than the TAR here, and the statement in lines 50-51 on the same page that the concepts of detection and attribution remain unchanged. Some clarification is required in one or other place. [Martin Manning (Reviewer's comment ID #: 155-60)]	Accepted. Sentence revised to make it clear that the current chapter includes detection and attribution.
9-117	A	5:6	5:6	Delete "system" [VINCENT GRAY (Reviewer's comment ID #: 88-949)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-118	A	5:10		"…attempt to place that work..." It is not clear what "that" refers to? What previous assessment precisely is being referred to here? [Govt. of United States of America (Reviewer's comment ID #: 2023-555)]	"That" refers to the areas of research described in the previous sentence. Wording slightly revised.
9-119	A	5:11	5:11	Replace "sometimes" by "usually" [VINCENT GRAY (Reviewer's comment ID #: 88-951)]	Rejected. "Usually" is too strong. See 9-121.
9-120	A	5:12	5:12	Delete "system" [VINCENT GRAY (Reviewer's comment ID #: 88-950)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-121	A	5:12		Should it say "quality of available climate models" instead of "available climate models". Also eliminate 'sometimes'. [Govt. of United States of America (Reviewer's comment ID #: 2023-556)]	Text revised. Sentence simplified by deleting "sometimes" and "available".
9-122	A	5:15	5:15	Paragrapg 5.1.1. is confused. First, you should avoid using the term "climate change" altogether, because it is defined, legally by the Framework Convention on Climate Change as exclusively concerbed with changes in greenhouse gases caused by humans. This is a very restricted definition which makes it impossible for scientists to study the climate before they have been able to attribute any possible cxause. It is no use claimimg that the IPCC hacve changed this definition. They have not. The FCCC definition is	Rejected. There is no particular reason for the IPCC to restrict itself to the UNFCCC definition of climate change. The definition given here remains unchanged from that used previously by the IPCC.

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				endorsed by an international treaty, and you cannot just change it like this. You have got to find a different way of talking about how the climate is changing [VINCENT GRAY (Reviewer's comment ID #: 88-952)]	
9-123	A	5:15	5:15	Replace Title by "Change and Variability of the Climate" [VINCENT GRAY (Reviewer's comment ID #: 88-953)]	Rejected. Important distinction between change and variability.
9-124	A	5:15	9:3	This chapter is almost a text book; it is very imformative but somewhat more than an assessment of the state of knowledge. It seems a bit late to be defining climate change and variability in chapter 9 and the history is meant to be in chapter 1. [Michael Manton (Reviewer's comment ID #: 157-29)]	Rejected. Useful to remind reader of definition at this stage.
9-125	A	5:17	5:17	The definition in the Glossary is unsatisfactory. It should begin by quoting the FCCC definition, which is, aftre all, part of a legally binding international treaty. It should then follow with a statement that because of the confusion caused by this definition the IPCC proposes to avoid using the term and replacing it with "change of climate" or "changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-954)]	Rejected. The definition is consistent with accepted scientifific usage of the term and with previous IPCC definition.
9-126	A	5:17	5:17	The definition in the Glossary does not even try to define "climate change" but only to tell us what it "refers to". It creates confusion by trying to draw a line between short term and long term changes but failing to do so. The difficulties of doing so are displayed by the statement "decades or longer". How much longer? Does this mean that claims that current changes are "unprecedented" over one thousand, ten thousand, a million years are wrong?. I suggest a limit with "geological eras"? [VINCENT GRAY (Reviewer's comment ID #: 88-955)]	Rejected. See 9-125.
9-127	A	5:17	5:17	I would like to get rid of the term "climate system". It cannot be distinguished from "climate"; so iif you keep the definition in the Glossary, say that you do noy recommend its use [VINCENT GRAY (Reviewer's comment ID #: 88-956)]	Rejected. The terms is useful, in common use, and not misleading.
9-128	A	5:17	5:17	You can then define "weather" as "Those changes in climate which are of comparatively short duration; such as" hourly, daily, weekly [VINCENT GRAY (Reviewer's comment ID #: 88-958)]	Rejected. No point in attempting to define weather as short-term climate. Would not increase comprehensibility.
9-129	A	5:17	5:17	You can then replace from "Climate change" on line 17 to "(see Glossary)" on line 19 with "Change of climate: a change in any part of the totality of influences on the surface of the earth and its neighbouring atmosphere over periods of decades or longer, extending to geological eras. . [VINCENT GRAY (Reviewer's comment ID #: 88-959)]	Rejected. See 9-125
9-130	A	5:17	5:24	The definition of "climate change" is so vague as to be useless. With this definition, no distinction between low-frequency variations and climate changes is possible. The way the term is used in the chapter (and in previous IPCC reports) is more consistent with a	Rejected. See 9-125.

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				view of climate change as a "forced" process. [Michael Mann (Reviewer's comment ID #: 156-70)]	
9-131	A	5:17	:19	Definition of clmate change needs improvement. For example, the word "change" is utilized twice in the definition of climate change. Needs improvement in the glossary as well. [Govt. of United States of America (Reviewer's comment ID #: 2023-557)]	Rejected. See 9-125.
9-132	A	5:19	5:19	Replace "Climate change" by "Change of climate" [VINCENT GRAY (Reviewer's comment ID #: 88-960)]	Rejected. Just a style preference.
9-133	A	5:19	5:19	Insert after "radiation", "cosmic rays, ocean instability, geological process" [VINCENT GRAY (Reviewer's comment ID #: 88-961)]	Rejected. Sufficient examples are already included.
9-134	A	5:20	5:20	Replace "occur" with "which vary and change" [VINCENT GRAY (Reviewer's comment ID #: 88-962)]	Rejected. Proposed change is ungrammatical.
9-135	A	5:20	5:23	Delete from "and" at end of line 20 to "activity" on line 23 [VINCENT GRAY (Reviewer's comment ID #: 88-963)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-136	A	5:23	5:23	Replave "climate change" with "changes in climate" [VINCENT GRAY (Reviewer's comment ID #: 88-964)]	Rejected. Just a style preference.
9-137	A	5:23	5:23	Replace "external forcings" with " changes on the composition of the atmosphere and of the earth's surface that began with the Industrial revolurion and can be attributed to human actiivity" [VINCENT GRAY (Reviewer's comment ID #: 88-965)]	Rejected. The aim of this chapter is to consider all external forcings, not just changes in atmospheric composition due to human activity.
9-138	A	5:24	5:24	Replace "internal" with "other" [VINCENT GRAY (Reviewer's comment ID #: 88-966)]	Rejected. It is important to try to distinguish between internally generated climate change and variability and changes caused by external forcings.
9-139	A	5:24	5:24	Delete "system" [VINCENT GRAY (Reviewer's comment ID #: 88-967)]	Rejected. Just a style preference.
9-140	A	5:27	5:27	Replace "extrenal influences" with "changes in atmospheric constituents" [VINCENT GRAY (Reviewer's comment ID #: 88-968)]	Rejected. See responses to 9-138 and 9-137.
9-141	A	5:27	5:27	Replace "internal climate variability" with "other climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-969)]	Rejected. See 9-137.
9-142	A	5:28	5:28	Replace "external' with "atmospheric composition" [VINCENT GRAY (Reviewer's comment ID #: 88-970)]	Rejected. See 9-137.
9-143	A	5:30	5:30	Replace "external' with "atmospheric composition" [VINCENT GRAY (Reviewer's comment ID #: 88-971)]	Rejected. See 9-137.

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9-144	A	5:33	5:33	Replace "external' with "atmospheric composition"" [VINCENT GRAY (Reviewer's comment ID #: 88-972)]	Rejected. See 9-137.
9-145	A	5:33	5:33	Replace "internal variability" with "the many other climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-973)]	Rejected. See 9-138.
9-146	A	5:35	5:35	Replace "climate change" by "changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-974)]	Rejected. Just a style preference.
9-147	A	5:35	5:35	Replace "natural variability" with "every influence on the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-975)]	Rejected. Important to point out that we need to understand role of naturally forced and internal variability.
9-148	A	5:35	5:37	Delete from "which' on line 35 to "scales" on line 37 [VINCENT GRAY (Reviewer's comment ID #: 88-976)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-149	A	5:35	5:35	I disagree with the contrast you make between "external influences" and "internal variability" It is a ruse to try and convince us all that only greenhouse gases cause "CHANGE and everything else is VARIABLE , The climate changes without greenhouse gases and you know it. You wish to conceal the very existence of "naural change" and also of changes caused by humans which do not involve greenhouse gases, such as the builkding of cities, changes in the land, and emission of energy. You just leave these out and so that your precious greenhouse gases remain as the only source of "climate change" You also want to pretend that ERl Niño ocean events are merely "internal variability" and draw attention away fgropm the fact that they have a major influence on the lower atmosphere. [VINCENT GRAY (Reviewer's comment ID #: 88-977)]	Rejected. The chapter specifically discusses (as does the Executive Summary) climate changes caused by natural external forcings, not just changes caused by changes in atmospheric composition. El Nino events are an example of how climate can vary due to internal variability.
9-150	A	5:36	5:36	Delete the definition of "internal climate processes" from the Glossary and refrain from using the term [VINCENT GRAY (Reviewer's comment ID #: 88-978)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-151	A	5:37	5:37	Replace "internal variability" with ."natural changes in the climate can also involve a variabity which" [VINCENT GRAY (Reviewer's comment ID #: 88-979)]	Rejected. Just a style preference.
9-152	A	5:37	5:37	Insert in front of "atmospheric processes "Besides long-term changes" [VINCENT GRAY (Reviewer's comment ID #: 88-980)]	Rejected. The next few sentences say this, and say it better.
9-153	A	5:37	5:37	Relace from "that generate" to "ranging" with "range" [VINCENT GRAY (Reviewer's comment ID #: 88-981)]	Rejected. Suggested deletion would lose the point that the processes mentioned are examples of processes producing internal variability. See also 9-149.

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9-154	A	5:39	5:39	The convection time scale is not instantaneous. A better example is cloud microphysics, such as condensation. [Michael Manton (Reviewer's comment ID #: 157-30)]	Accepted. Text revised.
9-155	A	5:41	5:41	Insert after "centuries" :besides any longer term changes" [VINCENT GRAY (Reviewer's comment ID #: 88-982)]	Revised. Specific time scales are no longer mentioned..
9-156	A	5:41	5:42	Delete from "components" on line 41 to "atmosphere" on line 42 [VINCENT GRAY (Reviewer's comment ID #: 88-983)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-157	A	5:41	5:42	The simple model of Hasselman is often mischaracterized, as it is here. It does not produce low frequency variability -- the oceanic heat capacity simply damps high frequency variability and leaves the low frequency variability already present in the atmosphere undamped. [Isaac Held (Reviewer's comment ID #: 105-35)]	Accepted. Text clarified.
9-158	A	5:42	5:43	Replace from ."internal" on line 42 to "by" on line 43 with "there are" [VINCENT GRAY (Reviewer's comment ID #: 88-984)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-159	A	5:44	5:55	Replace from "The climate's" on page 44 to "forcing" on line 46 with It is difficult to distinguish the effects of increases in greenhouyse gases from all the other climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-985)]	Rejected. If this sentence is deleted an important cause of uncertainty would be lost from the chapter.
9-160	A	5:48	5:48	Delete "Climate Change" [VINCENT GRAY (Reviewer's comment ID #: 88-986)]	Rejected. The topic IS "climate change detection and attribution".
9-161	A	5:48		A very well written introduction to the chapter! [Fons Baede (Reviewer's comment ID #: 9-4)]	Thank you.
9-162	A	5:49	5:49	Insert "of changes in theClimate" aftter "attribution" [VINCENT GRAY (Reviewer's comment ID #: 88-987)]	Rejected. Just a style preference.
9-163	A	5:50	5:50	Deletye "climate Change" [VINCENT GRAY (Reviewer's comment ID #: 88-988)]	Rejected. See 9-162.
9-164	A	5:50	5:50	Insert "of changes in the climate" aftter "attribution" [VINCENT GRAY (Reviewer's comment ID #: 88-989)]	Rejected. See 9-162.
9-165	A	5:50	6:6	Because of the excessively vague definition of "climate change" above, the definition of "detection" does not accord with usual practice, and the distinction between "detection" and "attribution" appears to be artificial. Although it is stated that "detection is the process of demonstrating that climate has changed in some defined statistical sense, without providing a reason for that change", the very next sentence annuls this statement by stating that the identification of change in observations is "based on the expected	Rejected. Incumbent on us to describe and assess the published literature which is based on these approaches.

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				responses to external forcing". This kind of language, going back to Hasselmann, has been confused and confusing in previous IPCC reports and in the literature on the subject. It would be good to use this opportunity to find a terminology that is more consistent with logic and practice. Detection and attribution are not really distinct. Attribution may be viewed as the standard model testing stage (e.g., Tiao and Box, J. Amer. Statist. Assoc., 1981) in regression modeling (which is what "optimal fingerprinting" that leads to "detection" really is). [Michael Mann (Reviewer's comment ID #: 156-71)]	
9-166	A	5:55	5:56	Replace from "random chance" on line 55 to "alone" on line 56 by "all the other climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-990)]	Rejected. Detection is the process of determing that a change has occurred that is unlikely to be explained by internal variability. This is an important distinction. The proposed change would make the sentence meaningless.
9-167	A	5:57	5:57	Replace "internal variability" by "the other influences" [VINCENT GRAY (Reviewer's comment ID #: 88-991)]	Rejected. See 9-166.
9-168	A	6:2	6:2	Delete "anthropogenic" [VINCENT GRAY (Reviewer's comment ID #: 88-992)]	Sentence has been revised
9-169	A	6:2	6:2	Insert after "influence" "from increases in greenhousde gases" [VINCENT GRAY (Reviewer's comment ID #: 88-993)]	Sentence revised.
9-170	A	6:3	6:3	Replace "anthropogenic forcing" by "changes in atmosphereic composition" [VINCENT GRAY (Reviewer's comment ID #: 88-994)]	Sentence revised.
9-171	A	6:4	6:4	In the legend of fig.9.4.1 change "anoamlies" with "anomalies" [Tiziano Colombo (Reviewer's comment ID #: 46-16)]	Accepted.
9-172	A	6:4	6:4	Replace "external forcing" with "changes in atmospheric composition" [VINCENT GRAY (Reviewer's comment ID #: 88-995)]	Rejected. This sentence is a general statement about what detection of any external influence means to impacts.
9-173	A	6:4	6:6	This last sentence seems to be irrelevant to the chapter [Michael Manton (Reviewer's comment ID #: 157-32)]	Rejected. It seems important to remind the reader that the detection of a signal and its importance from an impacts point of view are not necessarily the same thing..
9-174	A	6:8	6:8	Replace "external forcing" with "changes in atmospheric composition" [VINCENT GRAY (Reviewer's comment ID #: 88-996)]	Rejected. Models are used to investigate external forcings other than those associated with changes in

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					atmospheric composition.
9-175	A	6:8	:18	This whole paragraph does not really add anything of substance. [Govt. of United States of America (Reviewer's comment ID #: 2023-559)]	Rejected. The purpose of the paragraph is to describe the range of models that are used in different studies to determine the expected response to external forcing.
9-176	A	6:14	6:14	Replace "external forcing" with "changes in atmospheric composition" [VINCENT GRAY (Reviewer's comment ID #: 88-997)]	Rejected. See 9-137
9-177	A	6:15	6:18	Suggest suppressing these 4 lines as their added value is quite vague. It may also be understood that a warming trend associated with a change in rainfall is not the sign of an external influence. Another option could be to be more precise by introducing the idea of different time scales (changes in correlation sign between interannual and multidecadal timescales). [Govt. of France (Reviewer's comment ID #: 2010-58)]	Rejected. This example is included in response to comments on the FOD. The wording has been revised slightly to make it clear that the type of temperature/precipitation correlation that is discussed does not exist in all regions.
9-178	A	6:15	6:15	Delete "system" [VINCENT GRAY (Reviewer's comment ID #: 88-998)]	Rejected. Simple style preference.
9-179	A	6:15	6:18	My impression is that the negative correlation quoted here is characrteristic of semi-arid regions, with sub-polar regions, in contrast, showing the opposite. [Isaac Held (Reviewer's comment ID #: 105-36)]	Accepted. See changed wording in 9-177.
9-180	A	6:15	6:18	Precipitation and temperature are definitely *not* inversely correlated in most parts of the world (e.g., in the tropics, over midlatitude continents in summer etc.). The following inference that somehow temperature change that is not associated with rainfall change may indicate external influence is, in this generality, nonsense. [Michael Mann (Reviewer's comment ID #: 156-72)]	Accepted. See changed wording in 9-177.
9-181	A	6:16	:18	This relationship is very regionally dependent - e.g., warming and wetter conditions occur at high latitudes due to advection from lower latitudes. Probably should be removed, or at least have clear caveats. [Govt. of United States of America (Reviewer's comment ID #: 2023-558)]	Agreed. See changed wording in 9-177.
9-182	A	6:17	6:17	Replace "an external" by "a greenhouse gas" [VINCENT GRAY (Reviewer's comment ID #: 88-999)]	Rejected. See 9-137.
9-183	A	6:17	6:18	The association between temperature and precipitation anomalies seems weak to me. Isn't this mainly a midlatitude summer and tropical argument? In cold regions, warm anomalies are associated with increased precipitation. [Ronald J Stouffer (Reviewer's comment ID #: 258-16)]	Agreed. See 9-177.

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9-184	A	6:20	6:20	Replace "climate change" with "changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1000)]	Rejected. Simple style preference.
9-185	A	6:21	6:21	Replace "internal variability" with "the many other climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1001)]	Rejected. Important to distinguish between interannual and external causes.
9-186	A	6:25	6:25	Replace "internal variabity" with" otherclimate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1002)]	Rejected. See 9-185. The point is to remove the internal variability, not all influences on climate.
9-187	A	6:25	6:25	Add at end "but not the climate influences which could not be simulated by models" [VINCENT GRAY (Reviewer's comment ID #: 88-1003)]	Rejected. This sentence is about internal variability, not external "influences".
9-188	A	6:26	8:26	Delete "nor" [VINCENT GRAY (Reviewer's comment ID #: 88-1038)]	It is not clear what part of the text this comment refers to.
9-189	A	6:28	6:28	Replace "an external" by "a greenhouse gas" [VINCENT GRAY (Reviewer's comment ID #: 88-1004)]	Rejected. See 9-137.
9-190	A	6:28	:30	The example doesn't really help. Perhaps say, "Extreme events can occur in an unchanging climate." [Govt. of United States of America (Reviewer's comment ID #: 2023-560)]	The comment indicates that the reviewer does not really understand the statistical point that is being made here. We have tried to shorten and clarify the text, and have moved it into a better context (as part of the first paragraph of 9.1.2). We hope the point that is being made about the possibilty of spurious detection has now been communicated more clearly.
9-191	A	6:31	6:31	Insert "some of" after "reduce" [VINCENT GRAY (Reviewer's comment ID #: 88-1005)]	Rejected. Style preference. "reduce" implies "some of" (otherwise would have used "eliminates").
9-192	A	6:33	6:43	I think there is also a selection bias even in the approach used by most D&A studies in the sense that authors are more likely to publish positive D&A results i.e. those in which it is possible to reject the null hypothesis at some reasonably high level of probability. It is very rare to read a paper which says "we looked for an anthropogenic influence on X but didn't find any". In addition, the use of the 95%-tile to refect the null hypothesis means that at least one in ten positive D&A results must be due to chance mustn't they? [Matthew Collins (Reviewer's comment ID #: 44-21)]	Rejected. We are aware of the possibility of such a selection bias, but do not believe it is severe in d&a studies. Studies have been published even when they do not detect or attribute a change.
9-193	A	6:33	6:38	I found this discussion of "selection bias" confusing. "Fingerprinting" results in a	Rejected. Do not accept that this is

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				different kind of selection bias, in that only those patterns predicted by the model responses are looked for. One wouldn't have noticed the ozone hole if one followed this kind of program religiously. Tapio Schneider and I emphasized what we perceived as the need for model-independent detection strategies in an obscure short paper (Schneider, T., and I. M. Held, 2001: Discriminants of Twentieth-Century changes in earth surface temperatures. Journal of Climate, 14(3), 249-254) but evidently this is not a concern to most detection practitioners. The attribution of the ozone hole was not affected by the "selection bias" mentioned here, and I don't see how Sahel drought is fundamentally any different. Using the Sahel drought as an example in this context is not ideal in any case, since there is little question nowadays that African rainfall on these times scales is tied strongly to SSTs, and SST trends are one of the main foci of detection studies. [Isaac Held (Reviewer's comment ID #: 105-37)]	selection bias. This paragraph is about not being slavishly dependent on model responses. The Sahel example is an ideal example of this approach. Schneider and Held (2001) is now cited in a previous paragraph as an example of a purely diagnostic detection approach.
9-194	A	6:34	6:34	Replace "external" with "change in atmospheric composition" [VINCENT GRAY (Reviewer's comment ID #: 88-1006)]	Rejected. See 9-137.
9-195	A	6:42	6:42	add the relevant reference [Govt. of France (Reviewer's comment ID #: 2010-59)]	Sentence has been deleted.
9-196	A	6:43	6:43	Replace "external" with "change in atmospheric composition" [VINCENT GRAY (Reviewer's comment ID #: 88-1007)]	Rejected. See 9-137.
9-197	A	6:47	6:47	Insert after"(see Gloaary)" "but,of course, suc attribution can never be certain" [VINCENT GRAY (Reviewer's comment ID #: 88-1008)]	Rejected. This is implied by the term "most likely causes".
9-198	A	6:49	6:49	Add at end "but hardly anywhere else" [VINCENT GRAY (Reviewer's comment ID #: 88-1009)]	Rejected. Change would make sentence ungrammatical and meaningless.
9-199	A	6:50	:51	Drop (i) and move the other two components up to (i) and (ii), and in (i) change to 'demonstrate that a detected change is...' [Govt. of United States of America (Reviewer's comment ID #: 2023-561)]	Thank you for the suggestion. The wording has been simplified.
9-200	A	6:51	6:51	Replace "anthropogenic" with "greenhouse gas" [VINCENT GRAY (Reviewer's comment ID #: 88-1010)]	Rejected. Greenhouse gas forcing is not the only possible anthropogenic influence on climate.
9-201	A	6:51	6:51	Replace "natural forcing" with "all the other climate influences [VINCENT GRAY (Reviewer's comment ID #: 88-1011)]	Rejected. "natural forcing" does incude all external other climate forcings. The first step has already shown that an external forcing is required.
9-202	A	6:51	6:51	May I here complain about the use of the term "anthropgenic" to imply the influence of human emissions of grenhouse gase. The terrm, presumably should include human influences on the climate which do not invoivolve emissions of greenhouse gases, such as the building of cities, energy emission and land-use changes. You behave as if these	Nothing in the use of the term here implies that the term is being used to imply just greenhouse gases. That is why the term is used (otherwise would

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				influences can be totally ignored. You should only use "anthropogenic" when all human influences are being considered, but, on the whole, it should be replaced by "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1012)]	have said "greenhouse gases" or similar).
9-203	A	6:52	6:52	Replace "climate change" with "changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1013)]	Rejected. Simple style preference.
9-204	A	6:52	6:52	Replace "anthropogenic forcings" with "human climate influences". Surely "forcing" is what takes place at the top of the atmosphere. You cannot have "forcings" that are confined to the surface [VINCENT GRAY (Reviewer's comment ID #: 88-1055)]	Rejected. "anthropogenic forcings" is not used here.
9-205	A	6:55	6:57	This is a puzzling sentence. We have an "observed change" and an "estimated response to a hypothesized forcing". What then is a "hypothesized pattern of change estimated from observations"? [Fons Baede (Reviewer's comment ID #: 9-5)]	Thank you for pointing this out. Text revised.
9-206	A	7:5		Not sure if it should say "consistent" or "inconsistent". This sentence should be greatly simplified, along the lines of "the observed change is inconsistent with alternate hypotheses." [Govt. of United States of America (Reviewer's comment ID #: 2023-562)]	Yes, it should say "inconsistent"! Text revised.
9-207	A	7:14	7:14	Replace "internal climate variability" with "other climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1014)]	Rejected. The need for understanding internal variability in order to detect the effect of any external influence is clear. Other external influences are discussed explicitly elsewhere in that section
9-208	A	7:15	7:15	Replace " variability" with "other climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1015)]	Rejected. Proposed change would make sentence illogical.
9-209	A	7:16	7:16	Replace "internal nariability" with "the remaining climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1016)]	Rejected. See 9-207.
9-210	A	7:18	7:19	Replace "internal clmate variability" with "some part of the remaining climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1017)]	Rejected. See 9-207.
9-211	A	7:20	7:20	Replace "variability" with "influeneces" [VINCENT GRAY (Reviewer's comment ID #: 88-1018)]	Rejected. See 9-207.
9-212	A	7:21	7:21	Replace "internal variability" with "the many other climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1019)]	Rejected. See 9-207.
9-213	A	7:22	7:22	Replace "variability with "climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1020)]	Rejected. Here the focus is on whether or not the climate variations associated with internal processes such as El Nino

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					can be simulated.
9-214	A	7:23	7:23	Replace "variability with "climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1021)]	Rejected. See 9-213.
9-215	A	7:27	7:32	it would be interesting to add a comment on a specific variable like SLP for instance. [Govt. of France (Reviewer's comment ID #: 2010-60)]	Rejected. We chose not to elaborate here in the interest of space. The text indicates where examples will be presented.
9-216	A	7:27	7:32	This division between pattern (detection) and magnitude (attribution) has always struck me as a bit strange. The utility of this approach depends entirely on the assumption that the pattern of change is more robust and better understood than the magnitude and so takes precedence in the detection/attribution sequence. This may very well be the case, but it needs to be addressed more clearly and explicitly here. [Isaac Held (Reviewer's comment ID #: 105-38)]	Agreed. Paragraph rewritten to clarify.
9-217	A	7:29	7:29	Replace "climate change" with "changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1022)]	Rejected. Simple style preference.
9-218	A	7:34	7:37	Reducing structural uncertainty is clearly vital. However, I suspect that as we go deeper into the system it could become harder to give realistic estimates of the distributions of model parameters. This seems to me a flaw in the parameter ensemble approach. [Michael Manton (Reviewer's comment ID #: 157-33)]	Noted. It may be the case that the QUMP type approach will eventually be shown to be flawed, but we would contend that ensemble approaches do contribute to understanding of structural uncertainty.
9-219	A	7:43	7:43	Replace "climate change" with "changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1023)]	Rejected. See 9-217.
9-220	A	7:44	7:44	Insert before "temperature" "surface" [VINCENT GRAY (Reviewer's comment ID #: 88-1024)]	Rejected. Not just surface temperature change.
9-221	A	7:44	7:44	Insert after "century" "as a result.of the influence of proximity of measuring equipment to human activity rather than to the influence of increases in greenhouse gases"" [VINCENT GRAY (Reviewer's comment ID #: 88-1025)]	Rejected. The reviewer's statement is untrue (see Chapter 3).
9-222	A	7:45	7:45	Replace "robust" by "confirme" [VINCENT GRAY (Reviewer's comment ID #: 88-1026)]	Rejected. Simple style preference.
9-223	A	7:57	8:4	"below ... significance ... decrease in significance level": Should this be *above* and *increase* in significance level? Note that the significance level denotes the probability of *falsely* rejecting a null hypothesis, i.e., an error probability. [Michael Mann (Reviewer's comment ID #: 156-73)]	Thanks for this comment. This paragraph has been revised substantially to improve clarity.
9-224	A	8:4	8:4	Add at end "Unfortunately such expert judgemen tis not based on scientific principles and	Rejected. The reviewer's comment is

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				it can be unreliable" [VINCENT GRAY (Reviewer's comment ID #: 88-1027)]	untrue.
9-225	A	8:10	8:10	Replace "anthropogenic" with "human" [VINCENT GRAY (Reviewer's comment ID #: 88-1028)]	Rejected. Simple style preference.
9-226	A	8:12	:12	Insert after "century" " This statement did not specify the origin of the 'human influence', so it should not be interpreted as necessarily involving emissions of greenhouse gases, but would include the result.of the influence of proximity of the measuring equipment to human activity."" [VINCENT GRAY (Reviewer's comment ID #: 88-1029)]	Rejected. The proximity of human activity to measuring equipment was not the cause of the observed climate change reported in the TAR.
9-227	A	8:14	8:14	Insert after "influence". "such as the building of cities, the use of energy, and changes in the land" [VINCENT GRAY (Reviewer's comment ID #: 88-1030)]	Rejected. The TAR did not imply that cities were causing the detected climate change.
9-228	A	8:14	8:14	Replace "climate change" with "changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1031)]	Rejected. Simple style preference.
9-229	A	8:20	8:20	Add at end "But, of course, this did not include human contributions to changes in the climatees which do not involve emissions of greenhouse gases, such as the building of cities, energy emissiom, or changes in land use, which were not included in the models" [VINCENT GRAY (Reviewer's comment ID #: 88-1032)]	Rejected. None of these are causes of internal climate variability which is the focus of this sentence.
9-230	A	8:22	8:22	Replace "anthropogenic" with "human-emitted" [VINCENT GRAY (Reviewer's comment ID #: 88-1033)]	Rejected. Implies that all anthropogenic greenhosue gases have to be somehow "emitted" from human bodies.
9-231	A	8:24	8:24	Insert after "observations" "but increases in temperature around cities, where most measurements are made, is a better explanation" [VINCENT GRAY (Reviewer's comment ID #: 88-1034)]	Rejected. Temperature has increased almost everywhere, well away from cities and even in oceans.
9-232	A	8:24	8:24	Insert before "known" "some" [VINCENT GRAY (Reviewer's comment ID #: 88-1035)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-233	A	8:26	8:26	Insert before "warming". "surfae" [VINCENT GRAY (Reviewer's comment ID #: 88-1036)]	Rejected. Warming not just observed at surface.
9-234	A	8:26	8:26	Insert after "century" "which did not occur in the lowewr troposphere". [VINCENT GRAY (Reviewer's comment ID #: 88-1037)]	Rejected. Reviewer comment is incorrect.
9-235	A	8:27	8:27	Add at end "are probably related to a very recent warming spell". [VINCENT GRAY (Reviewer's comment ID #: 88-1039)]	Rejected. Proposed change would imply that "recent warming is caused by recent warming".
9-236	A	8:31	8:31	Replace "found" by "thought" [VINCENT GRAY (Reviewer's comment ID #: 88-1040)]	Rejected. The reviewer provides no supporting reasoning for the proposed

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					change.
9-237	A	8:32	8:32	Insert after "not" "most" [VINCENT GRAY (Reviewer's comment ID #: 88-1041)]	Rejected. . The reviewer provides no supporting reasoning for the proposed change.
9-238	A	8:33	8:33	Insert after "be" "vaguely" [VINCENT GRAY (Reviewer's comment ID #: 88-1042)]	Rejected. The reviewer provides no supporting evidence for the proposed change.
9-239	A	8:36	8:36	Replace "found to be robust to attempts" with " to some extent, able" [VINCENT GRAY (Reviewer's comment ID #: 88-1043)]	Rejected. The reviewer provides no supporting evidence for the proposed change.
9-240	A	8:40	8:41	Replace "climate changes" with "changes in climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1044)]	Rejected. Simple style preference.
9-241	A	8:41	8:41	Insert before "model" "some" [VINCENT GRAY (Reviewer's comment ID #: 88-1045)]	Rejected. Change would make sentence ungrammatical.
9-242	A	8:41	8:41	Replace "warming" by "temperature change" [VINCENT GRAY (Reviewer's comment ID #: 88-1047)]	"warming" replaced by "temperature rise".
9-243	A	8:46	8:46	Replace "internal climate variability" with "other climate influences,besides that of greenhouse gases"" [VINCENT GRAY (Reviewer's comment ID #: 88-1048)]	Rejected. Focus here is on internal climate variability, not all climate influences.
9-244	A	8:47	8:47	Replace "internal variance" with "alternative climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1049)]	Rejected. See 9-243.
9-245	A	8:48	8:48	Replace "an anthropogenic" by "a greenhouse gas" [VINCENT GRAY (Reviewer's comment ID #: 88-1050)]	Rejected. Would deny effect of ozone or aerosol impact on climate.
9-246	A	8:48	8:48	Insert after "climate" "but many imporatant influences, such as human ewffects on the surface, were omitted from the models" [VINCENT GRAY (Reviewer's comment ID #: 88-1051)]	Rejected. See 9-243.
9-247	A	8:49	8:49	Delete "external" All 'forcing" is "externaL' by definition [VINCENT GRAY (Reviewer's comment ID #: 88-1052)]	Rejected. "external" does remind readers that there is a separation between external factors and internal variability.
9-248	A	8:49	8:49	Delete "anthropogenic", There are uncertainties in all the aerosols, not just the human ones [VINCENT GRAY (Reviewer's comment ID #: 88-1053)]	Rejected. Uncertainties in natural aerosols from volcanoes is exlplicitly noted in next line.
9-249	A	8:52	8:52	Replace "warming" by "temperature change" It sometimes cools, and in the lower tropsphere, shows little overall change for extended periods [VINCENT GRAY (Reviewer's comment ID #: 88-1054)]	Rejected. The overwhelming 20th century signal is the warming.

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9-250	A	8:54	8:54	Delete "external" [VINCENT GRAY (Reviewer's comment ID #: 88-1056)]	Rejected. Pproposed change would make sentence illogical.
9-251	A	9:3	9:3	Add at end. "This statement suffered from two difficulties. First, the surface temperature record showed no 'warming' between 1950 and 1978, so a "warming" was only "observed" from 1978 onwards, a mere 20 years. Secondly, there is no mention of human influence, so the increases in greenhouse gases could be pureky natural; for example by an increase in the chief greenhouse gas, water vapour" [VINCENT GRAY (Reviewer's comment ID #: 88-1057)]	Rejected. The statement does not claim that there was continuous warming during the latter half of the 20th century. Secondly, the greenhouse gases referred to in the TAR statement were from anthropogenic sources, such as fossil fuel combustion and release of CFCs, as was detailed elsewhere in the report.
9-252	A	9:5	9:36	There is a road map at the front called a table of contents. This section can be deleted [Michael Manton (Reviewer's comment ID #: 157-34)]	Agreed. Section deleted.
9-253	A	9:5	9:36	If the chapter needs shortened, this may be a good place to start (9.1.4). The reader has the outline at the front of the chapter. Also, 9.1 in general seems too long. Some of the material could be moved closer to where it is used. This would also save a few sentences since things tend to be repeated later. [Ronald J Stouffer (Reviewer's comment ID #: 258-17)]	Agreed. Section 9.1.4 deleted.
9-254	A	9:7	9:7	Delete "by briefly" It is, by contrast, excessively long-winded [VINCENT GRAY (Reviewer's comment ID #: 88-1058)]	See 9-252 and 9-253.
9-255	A	9:21	9:21	Replace from "relatively" to "amounts" with "based on very limited, and poorly distributed" [VINCENT GRAY (Reviewer's comment ID #: 88-1059)]	Comment no longer relevant – see 9-252.
9-256	A	9:24		"and 8" should be dropped. If there are important observed changes discussed in Chapter 8 that are not currently referred to in Chapter 3, then Chapter 3 should be amended to include them. [Adrian Simmons (Reviewer's comment ID #: 242-127)]	Comment no longer relevant – see 9-252.
9-257	A	9:36		It is not clear what "its" refers to in this sentence. [Adrian Simmons (Reviewer's comment ID #: 242-128)]	Comment no longer relevant – see 9-252.
9-258	A	9:42	:45	Is all model analysis related to "radiative forcing"? Are there no model simulations that attempt to attribute changes to non-radiative anthropogenic influences, for example, anthropogenic changes in land cover. Line 40 should indicate, as the chapter title does, of climate response to radiative forcings. [Govt. of United States of America (Reviewer's comment ID #: 2023-563)]	Text has been revised
9-259	A	9:48	9:55	This paragraph is unclear. We suggest it is reviewed and possibly re-written. [Govt. of Australia (Reviewer's comment ID #: 2001-361)]	Text has been revised

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9-260	A	10:3	:4	Is it implicitly saying that the responses are linear and can be scaled? Could any references be provided. Are there model simulations and analysis that substantiates this statement? Could give the (Meehl et al 2004) reference here, but an overall consideration of how general this comment is should be considered. [Govt. of United States of America (Reviewer's comment ID #: 2023-564)]	Yes, revised to say explicitly and references added.
9-261	A	10:18		Para 9.2.1: I suggest to add a box explaining the difference between "forward" and "inverse". [Fons Baede (Reviewer's comment ID #: 9-6)]	Rejected - No space, also, this is a relatively well-known concept.
9-262	A	10:21	10:25	In Chapter 2, radiative forcing is given as "relative to pre-industrial times (~1750)" and this is what is quoted in the Summary for Policymakers also. The term radiative forcing as used in Chapter 2 and the Summary in most cases already implies that the quantity refers to the change since 1750, not the absolute value of the quantity. This could be reflected better in the wording of these lines. [Adrian Simmons (Reviewer's comment ID #: 242-129)]	Rejected - The definition is from the TAR and also used in Chapter 2. We stated Ch. 2 is looking at forcing since 1750, and have mentioned this several times
9-263	A	10:30		The figure in Ch02 shows a model estimate of radiative forcing - not a "response" to radiative forcing. [Martin Manning (Reviewer's comment ID #: 155-61)]	Revised according to suggestion
9-264	A	10:33	10:34	In its FOD, Chapter 2 did estimate total net forcing. That estimate has been deleted in this draft. If it is necessary for Chapter 9 to use a net value, it should be indicated as Chapter 9's interpretation of Chapter 2's information, not attributed to Chapter 2. [Lenny Bernstein (Reviewer's comment ID #: 20-67)]	Accounted for Net anthropogenic forcing estimate given in this section is now from the ch2 estimate, which backreferenced. Total forcing would be total anthropogenic and solar forcing, numbers for both of which are given consistent with chapter 2, so there are no more estimates not given in chapter 2.
9-265	A	10:33	10:34	How was this estimate of total net forcing derived? It does not appear in Chapter 2's discussion of radiative forcing. [Jeff Kueter (Reviewer's comment ID #: 137-61)]	See 9-264.
9-266	A	10:33	:34	In its FOD, Chapter 2 did estimate total net forcing. That estimate has been deleted in this draft. If it is necessary for Chapter 9 to use a net value, it should be indicated as Chapter 9's interpretation of Chapter 2's information, not attributed to Chapter 2. [Govt. of United States of America (Reviewer's comment ID #: 2023-565)]	See 9-264
9-267	A	10:33	:34	How was this estimate of total net forcing derived? It does not appear in Chapter 2's discussion of radiative forcing. [Govt. of United States of America (Reviewer's comment ID #: 2023-566)]	See 9-264
9-268	A	10:34	10:34	Replace "1.5±1" with "1.5±2" . 67% confidence intervals are unacceptable by truly	Rejected. See 9-8. 90% uncertainty

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				professional scientists. You must give 95% confidence intervals, and it must be imposed throughout this Report [VINCENT GRAY (Reviewer's comment ID #: 88-1060)]	ranges are widely used in the report, and meaning of ranges is given.
9-269	A	10:34	10:34	Delete "(67% confidence interval)" [VINCENT GRAY (Reviewer's comment ID #: 88-1061)]	See 9-8.
9-270	A	11:1	11:2	How likely is it that forward calculations yield a net RF close to zero or negative? [Fons Baede (Reviewer's comment ID #: 9-7)]	Information now given in Ch. 2, and in the revised section in ch9.
9-271	A	11:1	:8	Clarify and simplify the paragraph. [Govt. of United States of America (Reviewer's comment ID #: 2023-567)]	Text has been revised.
9-272	A	11:4	11:4	Delete "Hiwever" [VINCENT GRAY (Reviewer's comment ID #: 88-1062)]	Rejected. Would confuse reader.
9-273	A	11:4	11:4	Replacve "would be impossible" with "is not easy" [VINCENT GRAY (Reviewer's comment ID #: 88-1063)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-274	A	11:4	11:8	I find this argument hard to follow and if correct it seems to be quite important. Can you clarify/ elaborate the logic here. [Martin Manning (Reviewer's comment ID #: 155-62)]	It is important! Text has been clarified
9-275	A	11:5	11:6	Replace from "explanations" on line 5 to "both" on line 6 with "of" [VINCENT GRAY (Reviewer's comment ID #: 88-1064)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-276	A	11:8	11:8	Add at end "However, if the warming of the ocean is merely periodsic, and the models have omitted imporatnt climate influences, ther is a possibility that it is true" [VINCENT GRAY (Reviewer's comment ID #: 88-1065)]	Rejected. Statement would imply that we know nothing about the physics of the atmosphere-ocean system.
9-277	A	11:13	11:15	Awkward sentence. Also, what is special about ice phase? Is this the only problem climate models have? An alternate version could be "...forward calculation has not evaluated all forcings and feedbacks." [Govt. of United States of America (Reviewer's comment ID #: 2023-568)]	Accepted
9-278	A	11:25		also assumes ocean heat uptake is correct (for transient response). [David Rind (Reviewer's comment ID #: 214-88)]	Added qualification
9-279	A	11:25		Also assumes ocean heat uptake is correct (for transient response) [Govt. of United States of America (Reviewer's comment ID #: 2023-569)]	Added qualification
9-280	A	11:35	:45	Very un-illuminating paragraph….makes for needlessly difficult reading. [Govt. of United States of America (Reviewer's comment ID #: 2023-570)]	Text has been revised.
9-281	A	12:25	12:27	"pre-industrial and mid-Holocene": confusing terminology,since the mid-Holocene is pre-industrial. Better "early 19th century" for pre-industrial.	Agreed. We improved the terminology.

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				[Michael Mann (Reviewer's comment ID #: 156-75)]	
9-282	A	12:33	12:33	"solar insolation" is redundant [Michael Mann (Reviewer's comment ID #: 156-74)]	Accepted. The term Solar radiation or insolation are used instead.
9-283	A	12:41	12:47	There are several references here to "the pre-industrial". But the LGM and the mid-Holocene climates are both pre-industrial. If "radiative forcing" is used, as in Chapter 2, to refer to the difference from the 1750 value, then the references here to the "pre-inustrial" can be avoided by talking about "radiative forcing" rather than "radiative perturbation...since the pre-industrial". [Adrian Simmons (Reviewer's comment ID #: 242-130)]	Accepted. The term forcing is now adopted.
9-284	A	12:50	12:51	The seasonal changes in solar forcing for the Southern Hemisphere are large and should be mentioned here. [Bette Otto-Bliesner (Reviewer's comment ID #: 193-4)]	Accepted, value provided in the text.
9-285	A	13:3	13:6	Suggest adding to the text that it is the average of present day solar irradiance variations (to take into account the 11 year solar cycle). I think Chap 2 has some good examples [Gareth S. Jones (Reviewer's comment ID #: 121-76)]	Rejected. This part builds on chapter 2 where details are provided.
9-286	A	13:4	13:5	Please convert the solar irradiance estimates to estimates of radiative forcing. [Govt. of Finland (Reviewer's comment ID #: 2009-108)]	Accepted. Information provided
9-287	A	13:4	:5	I am not sure this is consistent with Chapter 2 which gives (section 2.7.1.2.2) a most likely increase of 0.5 W m-2 from the Maunder Minimum to current cycle minimum and a 1.1 W m-2 (0.08%) increase from MM to current cycle maximum (or do they mean cycle mean?). Furthermore Fig.2.21 seems to show (pink curves) a range of 0.7 Wm-2 (cycle min) to 1.5 Wm-2 (solar max). This needs clarifying both here and in Ch.2. [Joanna Haigh (Reviewer's comment ID #: 95-10)]	Rejected. It is consistent. For simplicity we only keep the information relative to present day, for which we are more confident in the estimation of solar radiation.
9-288	A	13:9	13:9	Add ”negative” before ”forcing”. [Govt. of Finland (Reviewer's comment ID #: 2009-109)]	Done
9-289	A	13:11	13:17	What do the percentage values given here mean? Does the first number of 50% mean that the the largest estimates are bigger than the smallest by typically 50%, or are the smallest smaller than the largest by 50% or that the range is +/- 50% around some "mean"? Each of these gives different ranges. Re-phrase how the uncertainty is described. [Gareth S. Jones (Reviewer's comment ID #: 121-77)]	Accepted, text has been revised.
9-290	A	13:11	13:17	There is indeed quite a bit of agreement on timing of many large volcanic eruptions, but not all. Robertson 2001 for instance has a reconstruction with significant volcanic eruptions either side of 1950, something most other reconstructions don't see. More discrepancies occur the further back you go in time. The confidence in the agreement in timing should be down-weighted here. [Gareth S. Jones (Reviewer's comment ID #: 121-78)]	We disagree, the text is consistent with the chapter 6 figure, and the discussion of agreement is explicitly about large eruptions. Also, Robertson's reconstruction is based on fewer ice core records than other reconstructions,

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					and consistently has larger uncertainties.
9-291	A	13:11	13:17	The estimate of the uncertainty in the magnitude of historic eruptions is too low. I just had a quick look at some reconstructions referenced in this chapter. For Pinatubo [1991] Ammann (2003) has OD greater than Sato (1993) by 20%. For Santa Maria [1903] Ammann (2003) is 225% larger than Robertson (2001). For Krakatoa [1883] Sato (1993) is 75% larger than Robertson (2001). For radiative forcing estimates the radiative effects have to be calculated either offline or within a climate model and so extra dependancies enter the calculations, so that uncertainty surely cannot be smaller than the optical depth/volcanic magnitude uncertainty. For Pinatubo 1993 Crowley (2000) has a forcing 70% bigger in magnitude than Tett (2002), for Santa Maria 260% larger and for Krakatoa 52%. Other reconstructions not referenced in this chapter also show large differences. I recommend making clear the big difference between some of the reconstructions. [Gareth S. Jones (Reviewer's comment ID #: 121-130)]	Accepted, text has been revised crossreferencing chapter 6. However, note response to 9-290.
9-292	A	13:26	13:26	Jargon. Change "with a coupled mixed layer (q-flux) ocean" to "a atmosphere model coupled to a a simple mixed layer ocean model using heat flux adjustments?. [Ronald J Stouffer (Reviewer's comment ID #: 258-18)]	Accepted
9-293	A	13:31	13:31	Replace "produces" by "expected to produce" [VINCENT GRAY (Reviewer's comment ID #: 88-1066)]	Accepted
9-294	A	13:31	13:31	Insert after "troposphere" but temperature measurements in the troposphere have found extensive periods (1858 to 2004 for radiosondes, and 1978 to 1998 for satellites) where no overall rtempersture change is found. On the other hand" [VINCENT GRAY (Reviewer's comment ID #: 88-1067)]	Rejected. Statement would be out of place since this section is about expected change in response to forcing
9-295	A	13:31	13:31	Insert after "stratosphere" " could be influenced by greenhouse gases" [VINCENT GRAY (Reviewer's comment ID #: 88-1068)]	Rejected. Suggested addition does not make any sense.
9-296	A	13:31	13:31	Delete "and" [VINCENT GRAY (Reviewer's comment ID #: 88-1069)]	Rejected. Suggested deletion would make the sentence ungrammatical.
9-297	A	13:31	13:35	The land-sea contrast and hemispheric asymmetry in question is only associated with the transient response to greenhouse gas forcing, and is not true of the equilibrium response. This statement requires clarification. [Michael Mann (Reviewer's comment ID #: 156-1)]	clarified
9-298	A	13:33	13:33	Insert after "921c)" "due to the increased heating in winter and at night in urban areas" [VINCENT GRAY (Reviewer's comment ID #: 88-1070)]	Rejected. There is no evidence to suggest that the expected increased northern hemisphere warming would be due to night time warming in urban areas.

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9-299	A	13:37	13:37	Delete from "thereby" to "warming". There is no evidence of consistent "warming" [VINCENT GRAY (Reviewer's comment ID #: 88-1071)]	Rejected. This describes an expected pattern of response.
9-300	A	13:39	13:38	is" by "ought to" [VINCENT GRAY (Reviewer's comment ID #: 88-1072)]	Accepted, text has been revised similar to suggestion
9-301	A	13:40	13:40	Add at end "but temperature measurements in the troposphere have failed to detect any such warming" [VINCENT GRAY (Reviewer's comment ID #: 88-1073)]	Rejected. The reviewer has confused the expected pattern with the observed pattern of change.
9-302	A	13:44	13:44	Insert before "greenhouse" "the supposed" [VINCENT GRAY (Reviewer's comment ID #: 88-1074)]	Rephraased.
9-303	A	13:44	13:44	Delete "to the response to greenhouse warming" It is redundant [VINCENT GRAY (Reviewer's comment ID #: 88-1075)]	Rejected. Reviewer has not followed the reasoning in this short sentence.
9-304	A	14:16	14:32	This description of the analyses shown in fig 9.2.3 is confusing. [Fons Baede (Reviewer's comment ID #: 9-8)]	Clarified
9-305	A	14:21		Between "is" and "larger" add" a factor of three" [Fons Baede (Reviewer's comment ID #: 9-9)]	Added quantification
9-306	A	14:23	14:25	How do I understand this statement from fig 9.2.3? To me there seems to be very little agreement between ISCCP FD and ERBS. [Fons Baede (Reviewer's comment ID #: 9-10)]	Text has been revised to clarify.
9-307	A	14:27		What is meant by "trend" here and in the following lines? [Fons Baede (Reviewer's comment ID #: 9-11)]	Text has been revised to clarify.
9-308	A	14:36	14:37	This does not seem to make sense. The figures only show the (temperature) response to forcing so can not be used to show that these patterns are different from the forcing patterns (which are not shown). [Martin Manning (Reviewer's comment ID #: 155-63)]	Text revised
9-309	A	14:37		and primarily due to the strength of the feedbacks relative to the initial forcing. [David Rind (Reviewer's comment ID #: 214-89)]	See 9-310.
9-310	A	14:37		…and primarily due to the strength of the feedbacks relative to the initial forcing. [Govt. of United States of America (Reviewer's comment ID #: 2023-571)]	Added
9-311	A	15:16	15:16	"optimally consistent": This language of "optimality", although ingrained in the field, is vacuous. The regression coefficients estimated in "optimal fingerprinting" are not generally optimal; they are optimal *conditional* on the choice of some truncated representation of the fields of interest (truncated spherical harmonics, principal components, etc.). Since these truncated representations are not "optimal" in any general sense, neither are the "optimal fingerprints" etc. throughout the chapter, "optimal fingerprinting": In fact, the often severe restriction to large spatial scales or linear trends may be decidedly suboptimal for detecting, for example, aerosol signals in the	Accepted, text has been revised and further "optimals" in the chapter have been checked and replaced, or clearly placed in context.

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				observational records.I suggest removing "optimal" throughout the chapter. Statisticians and mathematicians will find the term confusing. If the authors insist on keeping it, it must be qualified as conditionally optimal; otherwise the statements are false. [Michael Mann (Reviewer's comment ID #: 156-76)]	
9-312	A	15:28		After "But it is not yet clear.." add: "that any can be excluded,". [Fons Baede (Reviewer's comment ID #: 9-12)]	Text revised.
9-313	A	15:56	16:3	Should also mention here, or elsewhere in the chapter, the additional uncertainty due to variations in the spectral composition of solar radiation which will affect the vertical structure of the solar impact. [Joanna Haigh (Reviewer's comment ID #: 95-11)]	done
9-314	A	16:14	9:14	Delete "It has been shown" and start sentence at " For most forcings…" [Govt. of Australia (Reviewer's comment ID #: 2001-362)]	Accepted, text has been deleted
9-315	A	16:14	16:17	There seems to be a bit too much generalisation here. The generalisation about most forcings have similar efficacies for a given model is not supported by the available evidence. According to Table 2.23 (p157 chap2) only two studies look at more than two forcings and the ranges are not what I would call "similar". Also "most forcings" is repeated unecessarily in the sentence [Gareth S. Jones (Reviewer's comment ID #: 121-79)]	Accepted. Text has been revised, generalization is omitted now.
9-316	A	16:14	:15	Should the sentence say "… It has been shown that for a range of forcings, the global …" The way it is written sounds like difference forcing agents have same per unit response. Clarify the sentence. [Govt. of United States of America (Reviewer's comment ID #: 2023-572)]	Specific statement has been deleted
9-317	A	16:15		This is not the right definition of "efficacy" See: See Ch 2, page 2-3, lines 16-21. [Fons Baede (Reviewer's comment ID #: 9-13)]	Statement has been deleted
9-318	A	16:25	16:28	I think this sentence could be better constructed. It is rather tricky to understand. If I understand the sentence correctly then basically it is saying that for an uncertain amplitude forcing a scaling factor different from 1 could provide information about the uncertainty of that forcing? Whatever it is being attempted to be stated, a scaling factor significantly different than 1 does not necessarily tell you anything about the forcing amplitude. If the original forcing has large uncertainty the scaling amplitude can't be used to constrain the forcing without having some confidence in the model/reality efficacy for that forcing. [Gareth S. Jones (Reviewer's comment ID #: 121-80)]	Reworded
9-319	A	16:28	16:28	missing point [Pascale DELECLUSE (Reviewer's comment ID #: 58-45)]	Done

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9-320	A	16:50	16:50	correct ".." [Pascale DELECLUSE (Reviewer's comment ID #: 58-46)]	Done
9-321	A	17:0		section 9.2.4. Ok this is a controversial question. What can forcing estimates from inverse calculations actually be used for? It may seem to be useful to deduce from detection studies etc some limits on not well understood forcings. But those numbers can't then be used in any modelling studies covering the same period as that will become a circular argument i.e. the model will look similar to the observations because a forcing was chosen to best fit the observations. As large forcing uncertainties can inhibit accurate knowledge of the sensitivity (as stated) then producing smaller ranges from inverse calculations may cause a false increase in the confidence in the sensitivity range. It would be very helpful to more clearly explain what these estimates can be used for and what they cannot be used for. [Gareth S. Jones (Reviewer's comment ID #: 121-81)]	Rejected. These are not used in a circular context. However, meaning of inverse estimates has been clarified now.
9-322	A	17:2	17:5	Re-order sentence to make statement positive. [Govt. of Australia (Reviewer's comment ID #: 2001-363)]	Text revised
9-323	A	17:2	:4	"…, this does not strongly affect estimates …" In line of comments made prior to this, i.e., spatial pattern of greenhouse warming and aerosol cooling cannot be distinguished, how is this statement justified? Issue and sentence should be clarified. [Govt. of United States of America (Reviewer's comment ID #: 2023-573)]	Text revised.
9-324	A	17:14	17:16	Change "Forward model estimates of 20th century radiative forcing suggests that it is very likely that the total change in forcing is positive but less than 2.58 W/m2." to "Forward model estimates of 20th century radiative forcing indicate that the total change in forcing is positive and very likely less than 2.58 W/m2." [Govt. of Australia (Reviewer's comment ID #: 2001-364)]	The final estimates from chapter 2 are used now, and referenced to chapter 2. These numbers do give a very high probability for forcings being positive
9-325	A	17:16	17:16	Chapter 2 has the total RF as 2.59 W/m2 [Michael Manton (Reviewer's comment ID #: 157-36)]	Total RF from chapter 2 has been updated.
9-326	A	17:16	17:29	It would be helpful if the forcing estimates for well mixed greenhouse gases could be summarized here in addition to the total forcing (2.58) and aerosol forcing (-1.7) estimates. [Brian Soden (Reviewer's comment ID #: 245-10)]	Rejected, too specific for the summary section of 9.2, details are in the body of the section and chapter 2.
9-327	A	17:16		Would be useful to add a cross-chapter reference to the right part of Ch02 for the value of 2.58 W/m2. [Martin Manning (Reviewer's comment ID #: 155-64)]	Done
9-328	A	17:18	17:21	This would be clearer if you said that (1) the forward calculations shown in Ch02 are not inconsistent with the inverse results but (2) they indicate a greater likelihood of values more negative than -1.7. [Martin Manning (Reviewer's comment ID #: 155-65)]	Done

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9-329	A	17:24	17:24	It is not clear why uncertainty in aerosol forcing need be a major contributor to differences in projections of future climate change, if these projections determine climate senstiivity without trying to fit the 20th century record explicitly. [Isaac Held (Reviewer's comment ID #: 105-39)]	Text has been revised to be more clear.
9-330	A	17:41	17:41	".." [Pascale DELECLUSE (Reviewer's comment ID #: 58-47)]	Fixed
9-331	A	17:52	17:52	Replace "climate change" with "changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1076)]	Rejected. Simple style preference.
9-332	A	17:53	17:53	Replace "climate changes" with "changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1077)]	Rejected. See 9-331
9-333	A	17:53	17:53	Insert before "These" "Some of" [VINCENT GRAY (Reviewer's comment ID #: 88-1078)]	Rejected. The role of external forcings in these climate variations is welll established.
9-334	A	17:56	17:56	Insert after "variability" "and change" [VINCENT GRAY (Reviewer's comment ID #: 88-1079)]	Rejected. The sense of this sentence is clea as writtenr.
9-335	A	17:57	17:57	Replace "anthropogenic" by "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1080)]	Rejected. Simple style preference.
9-336	A	17:57	17:57	Replace "natural external forcings" with "other climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1081)]	Rejected. This change would make the sentence nonsensical.
9-337	A	18:0	20:	Section 9, pages 18-20 discuss climate change trends over the past 1000 years. It notes that 1675-1850 was a period of very cold temperatures, well below the average for the millenium. It further acknowledges that "it is not possible to simulate the large warming during the 20th century without anthropogenic forcing, stressing the importance of human activity to global warming." The second clause in that sentence (after the comma) reminds me Type I statistical errors. If the null hypothesis is "climate change is substantially caused by natural forces," then the report is rejecting that null. But what if that null is true? The report does not appear to look for the causes of the cooling during the 1675-1850 period. If there was not an anthropogenic cause for the historical cooling, what was the cause? Could it be that there were non-anthropogenic forces at work prior to the Industrial Revolution and the instrumental age of temperature measurement? Further, if human-activity-focused climate change models cannot retrocast historical climate changes, then why are we so confident in them? If we cannot answer these hypotheses, then our Type I error of "a jury convicting an innocent defendant" has extremely costly and disastrous consequences. The basic contention is that natural variability has an unknown magnitude that could have produced the colder conditions in the past and have some influence in the present. [Govt. of United States of America (Reviewer's comment ID #: 2023-574)]	Rejected. The reviewer appears to have taken bits of text out of context. The particular sentence that the reviewer appears to draw on (p20, lines 14-17) in fact describes the ability of models to simulate the cold conditions of 1675-1715 when driven with natural forcing reconstructions. So it would appear that the very sentence that is quoted describes our understanding of possible causes. The subseqneut sentence simply goes on to note that these same models can not also simulate 20th century conditions without an additional forcing.

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9-338	A	18:0		section 9.3.2 isn't LGM covered adequately in chap 6? [Gareth S. Jones (Reviewer's comment ID #: 121-82)]	Rejected, LGM is also part of the sensitivity sections in chapter 9
9-339	A	18:11	18:11	Replace "anthropogenic forcing" with "human activity" [VINCENT GRAY (Reviewer's comment ID #: 88-1082)]	Rejected. The reviewer provides no supporting reasoning for the proposed change.
9-340	A	18:12	19:13	Replace "increrases our confidence in" by "improves" [VINCENT GRAY (Reviewer's comment ID #: 88-1083)]	Rejected. Simple style preference.
9-341	A	18:14	18:14	Replace "anthropogenic": by "human-induce" [VINCENT GRAY (Reviewer's comment ID #: 88-1084)]	Rejected. No reason given for change.
9-342	A	18:14	18:15	Deledte from "In addition" on line 14 to "variability" on line 15. I don't understand how errors can be so "helpful" [VINCENT GRAY (Reviewer's comment ID #: 88-1085)]	Rejected. The residuals referred to in the text are a source of information concerning the climate's natural internal variability.
9-343	A	18:21	18:27	This paragraph explains that EMICs and AOGCMs have improved our understanding but it certainly lcks of specificity. Could at least one example, or one reference be provided to clarifiy the statement ? [Pascale DELECLUSE (Reviewer's comment ID #: 58-48)]	Accepted. The paragraph has be deleted and information on model used for these time period included above
9-344	A	18:21	18:24	Should probably say what an AOGCM is on first using it. Some other chapters do this, others don't! [Gareth S. Jones (Reviewer's comment ID #: 121-101)]	Comment no longer applies – text deleted.
9-345	A	18:30	18:47	there is still little understanding of the tropical response during the LGM. Cooling of 2°C of the global oceans is not sufficient to reproduce the land evidence, which implies cooling on the order of 5°C. This lack of concensus on tropical understanding is a very important feature - failure to mention it in these paragraphs makes them highly misleading. [David Rind (Reviewer's comment ID #: 214-90)]	Rejected. It is indeed an important feature, but to what extent this is a problem is unclear. This topic is also covered in chapter 6. The full evaluation of new simulations is not yet ready, and data have been revisited and show a range of results. There is thus no need to add more on LGM in chapter 9.
9-346	A	18:37	:47	There is still little understanding of the tropical response during the LGM. Cooling of 2°C of the global oceans is not sufficient to reproduce the land evidence, which implies cooling on the order of 5°C. This lack of consensus on tropical understanding is a very important feature - failure to mention it in these paragraphs makes them highly misleading. [Govt. of United States of America (Reviewer's comment ID #: 2023-575)]	See 9-345
9-347	A	19:1		Change "most notable changes in climate" to "most notable indication for changes in	Accepted.

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				climate" [Govt. of United States of America (Reviewer's comment ID #: 2023-576)]	
9-348	A	19:12	19:13	"less consistent" Between what and what? In the future? I am lost. [Ronald J Stouffer (Reviewer's comment ID #: 258-19)]	Accepted. Sentence clarified
9-349	A	19:19	19:21	How confident are you that vegetation feedbacks amplify the response? [Ronald J Stouffer (Reviewer's comment ID #: 258-20)]	Text revised.
9-350	A	19:28	19:33	I am not entirely convinced that there is a consistent picture of models replicating the reduced ENSO variability during the mid-Holocene. Although some studies have shown a weakening, only a handful of studies have been performed and the direct comparison with the palaeo-archive has been limited. Perhaps this is the meaning of the "controversial" qualifier. It might be better to conclude that "A small number of studies have analysed interannual variability in simulations of mid-Holocence climate and some show reduced variability in precipitation (refs) and less frequent and intense ENSO events (refs), although there is no consistency in the magnitude of the response and changes in associated teleconnections." Perhaps though it would be possible to add "The ability of models to reproduce the mid-Holocene ENSO variability, as quantified by direct comparison with the palaeo-record, may provide a constraint on model predictions of changes in ENSO under future scenarios." [Matthew Collins (Reviewer's comment ID #: 44-11)]	Patially accepted. There is agreement in several publication on reduced ENSO magnitude. There is less agreement on the amplitude, and mechanisms. There is still a limited number of publications. The paragraph has been reorganised to better reflect this, together with the remarks. A more detailed discussion of ENSO cannot be included here, but can be found in chapter 6.
9-351	A	19:29	:30	Should say "…less frequent and less intense…" if that is what is meant. [Govt. of United States of America (Reviewer's comment ID #: 2023-577)]	Accepted, but the sentence has been revised, because we are more confident on the results for the intensity than for frequency.
9-352	A	19:31		ambiguous statement. Is it "less frequent and less intense ENSO" or "less intense and more frequent ENSO "? [Pascale DELECLUSE (Reviewer's comment ID #: 58-49)]	Accepted.see resp 351. Sentence has been reworded
9-353	A	19:33		Should it say "A key element for less frequent and less intense ENSO is the Bjerknes mechanism." Clarify which ENSO response is being referred to. [Govt. of United States of America (Reviewer's comment ID #: 2023-578)]	Accepted. Sentence has been clarified
9-354	A	19:34	19:36	The increased solar heating will not necessarily lead to more warming in the west rather than the east. Model simulations of mid-Holocence (and future changes) simulate a wide range of east-west gradient responses and this is not the only mechanism whereby ENSO variability may be surpressed. [Matthew Collins (Reviewer's comment ID #: 44-12)]	Accepted. Paragraph reworded.
9-355	A	19:34		note that this paragraph buys into the idea that warming will be greater in the western than eastern Pacific due to the importance of dynamics in the eastern Pacific. However, in Chapter 10 and elsewhere, results are shown indicating the majority of coupled models	Accepted. The paragraph was reworded to better show that this is not necessarily supported by all

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				respond to warming (from GHG) by warming the eastern Pacific more than the western. This paragraph thus inadvertently contradicts the future model projections. This topic remains controversial, and by wholeheartedly embracing one particular conclusion here, it puts this section at odds with other parts of the report. [David Rind (Reviewer's comment ID #: 214-91)]	simulations, but by the work of Clement (2000;2004).
9-356	A	19:34		This paragraph buys into the idea that warming will be greater in the western than eastern Pacific due to the importance of dynamics in the eastern Pacific. However, in Chapter 10 and elsewhere, results are shown indicating the majority of coupled models respond to warming (from GHG) by warming the eastern Pacific more than the western. This paragraph thus inadvertently contradicts the future model projections. This topic remains controversial, and by wholeheartedly embracing one particular conclusion here, it puts this section at odds with other parts of the report. [Govt. of United States of America (Reviewer's comment ID #: 2023-579)]	Accepted see 9-355
9-357	A	19:55	19:57	Do the dates actually match the coldest periods of the last millennium? Fig 6.10 seem to show colder periods (in some reconstructions) in the 13th and 14th centuries and late 16th century. [Gareth S. Jones (Reviewer's comment ID #: 121-83)]	Agreed. Sentence reworded.
9-358	A	19:57	20:3	It seems inappropriate to single out the "Mann et al 1999" reconstruction in this context. In the TAR, 3 different reconstructions (Mann et al, 1999; Jones et al, 1998; Briffa et al, 1999) were shown which each indicated similar levels of variability within the indicated uncertainties. Other very similar reconstructions (Crowley and Lowery, 2000) were published shortly thereafter. Some newer reconstructions from entirely independent sources (e.g. Glacial mass balance records; Oerlemans, 2005) indicate a very similar level of variability to these earlier reconstructions. An objectively defensible version of this statement would be something to the effect that many reconstructions published since the TAR indicate greater variability that those (Mann et al, 1999; Jones et al, 1998; Briffa et al, 1999) highlighted in the TAR, but some indicate very similar levels of variability. The subsequent discussion of uncertainty then follows quite reasonably. [Michael Mann (Reviewer's comment ID #: 156-2)]	Agreed. Sentence has been changed, refering to the TAR and chapter 6. It is not possible to include additional references.
9-359	A	20:14	20:15	More detail is needed here as this sentence is too vague. Any period can be described as "one of the coldest periods", be more precise. Looking at Figure 6.13d the 1675-1715 period does not jump out as a particularly cold period for all the models, unlike periods around 1470 and1820. The authors should be very careful about any claims of qualitive agreement between models for a period in the middle of 1500-1899 the average of which is used as the reference. [Gareth S. Jones (Reviewer's comment ID #: 121-84)]	Agreed. Sentence has been modified. Comment on claims rejected, similar claims have also been made for different centering periods, and statement is weak anyway.
9-360	A	20:19	20:19	Add at end 'due to the proximity of temperature measuring equipment to increased	Rejected. The reviewer's statement is

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				human activity" [VINCENT GRAY (Reviewer's comment ID #: 88-1086)]	incorrect.
9-361	A	20:33	20:34	The use of "could" here is not reasonable. It implies that there is some uncertainty about this. There is not. Osborn et al (2006, now) have clearly shown that the originally undisclosed disequilibration error in the "Erik" ECHO-G simulation DOES explain the unrealistically warm conditions in that simulation during the early centuries of the millennium, as well as a long-term unphysical drift over the entire simulation. When Osborn et al (2006) remove the estimated erreonous drift and also account for the additional problem of the absence of tropospheric anthropogenic aerosol forcing in the "Erik" simulation, the result looks very much like all of the other simulations that have been done of the past millennium. [Michael Mann (Reviewer's comment ID #: 156-3)]	Accepted; Sentence reworded.
9-362	A	21:1	21:4	Suggest changing "anthropogenic" to greenhouse gases as the sulphate aerosol component to the anthro response pattern is quite different to GHG and solar. Also the patterns are similar on annual timescales, as the forcings are SW and LW there is quite a seasonal difference. i.e. The response to solar forcing is similar to the response to greenhouse gases on annual timescales. [Gareth S. Jones (Reviewer's comment ID #: 121-85)]	Accepted. Change done
9-363	A	21:4	21:4	Manabe and Wetherald noted this before the dawn of time. They should be referenced. [Ronald J Stouffer (Reviewer's comment ID #: 258-21)]	Accepted, citation added to section (which has been moved to 9.2)
9-364	A	21:10	21:10	correct "forcings" [Pascale DELECLUSE (Reviewer's comment ID #: 58-50)]	Rejected. Text was grammatically correct, so no change required.
9-365	A	21:24	:25	Should include some references from literature on decadal variability of ENSO. Generalize the community opinion. [Govt. of United States of America (Reviewer's comment ID #: 2023-580)]	Rejected, There is not enough information available for the last millenium from observation and simulations.
9-366	A	21:28	21:29	It should be noted here that the simulations described by Mann et al (2005) actually reproduce the changes in mean and variability indicated by the Cobb et al (2003) coral data. Moreover, it should be noted that the response in the model represents a combination of tropical volcanic and solar forcing, not just volcanic forcing. A better fit to the Cobb et al (2003) data is actually achieved when both forcings are used. [Michael Mann (Reviewer's comment ID #: 156-4)]	Accepted. The sentence has been reworded, but level of detail has to be limited based on space limitations, and to reflect overall scientific evidence in a balanced manner.
9-367	A	21:28	21:29	The phrasing here is misleading. It should be noted that Mann et al (2005) doesn't just suggest "the possibility of a link". It specifically reproduces the finding of Adams et al (2005) of a short-term El Nino-like response to explosive tropical volcanism based on forced experiments using the Cane-Zebiak model of tropical Pacific ocean-atmosphere dynamics. Moreover, it shows that the reconstructed changes in El Nin o variability and	See 366

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				mean state shown by the Cobb et al (2003) coral data are reproduced by the model when tropical volcanic and solar forcing is used to force the model. The reason for the poor correspondence with global forcing estimates found by Cobb et al (2003) was that that study did not distinguish between tropical and extratropical volcanic forcing (only the former matters for the tropical Pacific). [Michael Mann (Reviewer's comment ID #: 156-67)]	
9-368	A	21:29	21:30	Should also add that Robock 2000 discusses this in his review and mentions several studies that did not find a correlation. Also Collins, M. "Predictions of Climate following volcanic eruptions", Volcanism and the Earths Atmosphere, AGU 2004 examined ensembles of model simulations (HadCM3) of El Chichon and Pinatubo and found no link with either El Nino or La Nina Events. [Gareth S. Jones (Reviewer's comment ID #: 121-86)]	Accepted, citation for Robock added.
9-369	A	21:29	21:30	This assertion is made but not supported. Is it meant that the reconstructions of forcings are too uncertain? Or that the reconstructions of ENSO are too uncertain? Or that the models cannot yet be trusted to faithfully represent the mechanisms by which forcing might influence ENSO? Or some combination of the above? I can see some justification in any of these lines of reasoning, but the authors need to clarify the basis for the assertion nonetheless. [Michael Mann (Reviewer's comment ID #: 156-5)]	Accepted. Sentence reworded.
9-370	A	21:29	21:30	This is oddly worded. The "how" might be justified, but the "whether" certainly is not. The studies cited provide clear evidence that there is a relationship. There are no other studies cited here which contest the relationship in question. What might reasonably be considered uncertain, as discussed for example by Mann et al (2005), is the amplitude of the response, or the relative role of solar variability (for which the amplitude is far more uncertain). [Michael Mann (Reviewer's comment ID #: 156-68)]	See 9-369
9-371	A	21:32	21:34	Should add Stenchikov 2002 to this reference list. [Gareth S. Jones (Reviewer's comment ID #: 121-87)]	Stenchikov et al 2006 added.
9-372	A	21:36	21:38	Shindell et al (2001b) should be cited here [Michael Mann (Reviewer's comment ID #: 156-6)]	Rejected, we only keep here the 2003 publication, also, this is now crossreferenced to 9.2 where topic resides.
9-373	A	21:38	21:38	apart from Luterbacher et al. 2004 also Luterbacher et al. 2002 should be cited, as this reference relates to the NAO reconstruction: Luterbacher, J. , E. Xoplaki, D. Dietrich, P. D. Jones, T. D. Davies, D. Portis, J. F. Gonzalez-Rouco, H. von Storch, D. Gyalistras, C. Casty and H. Wanner, 2002: Extending North Atlantic Oscillation Reconstructions Back to 1500. Atmos. Sci. Lett., 2, 114-124, doi: 10.1006/ asle.2001.0044	Accepted

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				[Jürg Luterbacher (Reviewer's comment ID #: 151-12)]	
9-374	A	21:38	21:38	Shindell et al (2001b) also provided evidence from proxy-reconstructions and should be cited along with Luterbacher et al (2004). [Michael Mann (Reviewer's comment ID #: 156-7)]	Accepted
9-375	A	21:41	21:43	It would be appropriate to note here that Mann et al (2005) do find evidence for a role in solar variability, but it is 2nd order in its importance relative to tropical explosive volcanism. [Michael Mann (Reviewer's comment ID #: 156-69)]	Rejected. Mann et al (2005) discussed in previous paragraph.
9-376	A	21:42	21:43	Cite also Mann et al (2005) in this context. [Michael Mann (Reviewer's comment ID #: 156-8)]	Rejected. Mann et al. specifically discussed in the ENSO part.
9-377	A	21:48	21:48	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1087)]	Rejected. Simple style preference.
9-378	A	22:15	22:15	Replace "seen as benchmarks" with "routinely used to evaluate". [Govt. of Australia (Reviewer's comment ID #: 2001-365)]	Rejected. Routinely sounds excessive
9-379	A	22:22	22:22	correct "remains" [Pascale DELECLUSE (Reviewer's comment ID #: 58-51)]	Done
9-380	A	22:25	2:25	Insert after "variability" "and change" [VINCENT GRAY (Reviewer's comment ID #: 88-1088)]	Agreed. The reviewer is correct in stating that our understanding of climate change as well as climate variability has increased since the TAR (thanks for agreeing with us on this!).
9-381	A	22:33	22:33	correct "xplain" [Pascale DELECLUSE (Reviewer's comment ID #: 58-52)]	accepted
9-382	A	22:33	22:33	"explains" is mispelled. [Michael Mann (Reviewer's comment ID #: 156-9)]	accepted
9-383	A	22:39	30:9	This whole section is positively obsessed with the global mean surface temperature record, as if it was the only temperature record worth considering. Yet the "external forcing" to which you persistently allude takes place externally, at the top of the atmosphere, in the lower troposphere, and the first place to test its influence should be with temperature records in this region.There are two reliable records, the radiosonde record (which you try to dismiss as grossly inaccurate without good reasons) and the satellite record from MSU units, You make no attempt to simulate these records, because the results are in conflict with the studies on the surface, but they should have priority. The radiosonde records (Thorne et al 2005)and Angell 2006) both show no temperature change in the lower atmosphere from 1958 to 2004. The MSU record shows no	Rejected. This section is entitled "Global Scale Surface Temperature Change" so it focusses on global surface temperature. The free atmosphere is discussed starting at 9-35.

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				temperature change from 1978 to 1998, followed by a large El Niño peak in 1999 and a short irregular cooling spell since.. The attempt to convince us that these records are compatible with the surface record in Chapter 3 depends on plotting them altogether and trying to pretend thast the El Niño peak represents a long term trend which can be attributed to "external forcing" This argument is absurd. There is no evidence for the influence of "external forcing" in the place where it is supposed to happen, so that the contents of this section of this report are irrelevant [VINCENT GRAY (Reviewer's comment ID #: 88-1213)]	
9-384	A	22:39	30:9	This entire section is full of false conclusions because it gives incomplete consideration to several important influences on the climate and on the surface record which are not considered in any of the simulations. As a result, none of the simulations can be taken seriously. This defect is concealed by the use off misleading and ill-defined generalised entities such as "imternal variabilty" external forcing" "anthropogenic" and the like which I have tried to translate into actual climate influences. [VINCENT GRAY (Reviewer's comment ID #: 88-1214)]	Rejected. The reviewer provides no supporting evidence of this assertion.
9-385	A	22:39	30:9	The most important natural climate influence that has been omitted is the El Niño ocean phenom,enon with its counterpart El Niña which has a distyinct and obvious influence on all the temperature records. The Hadley people at one time used to "correct" for tts influence. Now, you decide to forget it completely, despite thew fact that it was responsible for the "highest annual mean temperature since records began" in 1999. The simulation of past temperatures without El Niño is meaningless [VINCENT GRAY (Reviewer's comment ID #: 88-1215)]	Rejected. El Nino causes climate variations, and its influence (and other internal processes) on climate variability is discussed elsewhere in the chapter.
9-386	A	22:39	30:9	Then you have failed to question any of the many problems with the actual data, which have been so scrutinised with the other temperature records. Peterson (2003. Journal of Climate Volume 16 pages 2941-2959) has shown that instrument errors are a major source of bias in the United Staes and , when allowed for, give a "corrected " record which shows little or no warming for the continental US (see Figure 3.2.3). A similar exerciose has recently been carried out in China (Zhao et al. 2005 Acta Metorologica Sinica Vol 19, pages 389-399). with a similar result. Unfortunately a similar correction process cannot be nade for most of the data making up the surface record bercause of insufficient numbers of stations for comparison purpose, but it is most proibable that the entire record would display very little "warming if this could be done [VINCENT GRAY (Reviewer's comment ID #: 88-1216)]	Rejected. The surface record has been examined carefully for many parts of the world and continues to reveal warming when these factors are taken into account. See Chapter 3.
9-387	A	22:39	30:9	Gray (2000. "The Cause of global Warming" Energy and Environment Volume 11, pages 613-639) has summarised the abundant evidence that weather station measurements are influenced by chnages in surrounding buildings, towns, vegetation, and energy usage. The claims made in Chapter 3 that these claims are "negligible do not answer this evidence	Rejected. Many others have identified that buildings etc affect weather station measurements. This is not the same as concluding that the global trends are

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				[VINCENT GRAY (Reviewer's comment ID #: 88-1217)]	biassed due to buildings. This question has been examined extensively in Chapter 3 and in previous IPCC Assessments.
9-388	A	22:39	30:9	McKitrick & Michaels (2004 "A test of corrections for extraneous signals in gridded surface temperature data", Climate Research Vol 20) made a statistical study on individual stations and on the supposedly "corrected" mean gloibal temperature record from 1979 to 2000 and found a significant influence of a range of socioeconomic factors such as population, energy usage and prosperity [VINCENT GRAY (Reviewer's comment ID #: 88-1218)]	Rejected. Many studies have indicated that the possible influence of such factors on global temperature increase is limited.
9-389	A	22:39	30:9	McKitrick and Michaels (2004) also found a significant upwards bias from the use of incomplete data. The data from Russia fron 1979 is partuicularly suspect, but nothing has been done to question it or to enquire how many other data are so contaminated [VINCENT GRAY (Reviewer's comment ID #: 88-1219)]	Rejected. The global temperature data have been examined extensively.
9-390	A	22:47	22:51	Change "2004" to "2005" wherever "2004" occurs. Chapter 3 now has warming of 0.78 °C ± 0.18 °C by 2005 relative to 1850-1919, and a warming of 0.17 °C per decade over 1979-2005. In early 2007, update to agree with Chapter 3 which will include the global surface temperature anomaly for 2006. [David Parker (Reviewer's comment ID #: 195-129)]	Accepted. Text changed to be in line with comments.
9-391	A	22:50	22:50	Insert after "record)" "However, drawing a linear curve through such irregular data cannot be justified. There were, essentially, four distinct phases; the first, from 1856 to 1910, showed a slight fall of 0.01°C, the second, from 1910 to 1942, showed a steady rise of 0.4°C, the third, from 1942 to 1978 showed a fall of 0.04°C, from 1978 to 1998 a fairly staedy rise of 0.45°C followed by an irregular fall to 2005.. It is very difficult to attribute any of these phases to external forcing. The first two took place when greenhouse gas concentrations were relatively low, and in any case, why should the influence suddenly occur in 1910? The third phase, a temperature fall,was at a time of increaseing greenhouse gas concentrations.For the fourth phase, 1978 to 1999 it is difficult to explain why greenhouse gases should suddenly be effective after being so long undetectable.and the 1999 peak is undoubtedly due to El Niño. The fall in temperature since 1999 is incompatible with a greenhouse gas explanation. The whole sequence is much better explained by the development of meteorological services over the period, and its influence from the proximity of measuring equipment to human development [VINCENT GRAY (Reviewer's comment ID #: 88-1089)]	Rejected. There is no evidence that the proximity of measuring equipment to human development or development of meteorological services has caused the non-linear variations the reviewer cites. Non-linear temperature variations are described starting at 22-55.
9-392	A	22:51	22:51	Insert after "(see Chapter 3)" " A statistical study by McKitrick and Michaels (2004) on the sequence from 1979 to 2000 showed that both individual records and the global average were significantly influenced by a variety of socioeconomic factors such as	Rejected. See 9-388.

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				increases in population. wealth, coal usage, and poor data quality", [VINCENT GRAY (Reviewer's comment ID #: 88-1090)]	
9-393	A	22:55	22:55	Insert after "(see Chapter 6)" "but this depends on comparing measurements made far from human activity with those much influenced by human activity. Also McIntyre and McKitrick (2003) have identified serious errors in some of the calculations" [VINCENT GRAY (Reviewer's comment ID #: 88-1091)]	Rejected. Multiple recontsructions of temperature by various groups reach this conclusion.
9-394	A	22:55	23:3	Delete from "Global mean" on page 22, line 55 to "(Figure 3.2.6)" on page 23, line 3 This consideration shou;ld appear immediately after the mention of the surface record on page 22 line 50; and I have provided a better and more cetatiled treatment there. [VINCENT GRAY (Reviewer's comment ID #: 88-1092)]	Rejected. Simple difference of opinion for where this description should be placed.
9-395	A	23:3	23:6	Transfer the whole section from "Since the mid 1970s" on line 3 to "ocean" on line 6; including the previoud two amendments to line 51 on page 22; inserting it after "period". .The material on "surface measurements should all be in one place. You should also start a new paragraph for "A large number of proxy reconstructions" [VINCENT GRAY (Reviewer's comment ID #: 88-1095)]	Rejected. Simple difference of opinion for where this description should be placed.
9-396	A	23:4	23:4	Insert after "period", due to better heating in urban areas at night and in the winter. [VINCENT GRAY (Reviewer's comment ID #: 88-1093)]	Rejected. Reviewer does not provide justification for the proposed change.
9-397	A	23:4	23:4	Delete "and" and capitalise "Warming" [VINCENT GRAY (Reviewer's comment ID #: 88-1094)]	Rejected. Simple style preference.
9-398	A	23:13	23:13	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1096)]	Rejected. simple style preference.
9-399	A	23:16	23:21	To the list of different forcings should be added the carbonaceous aerosols (BC/BB) and land use changes? [Gareth S. Jones (Reviewer's comment ID #: 121-88)]	Accepted. Text modified.
9-400	A	23:19		"model physics" to "model representations of physics" [Daithi Stone (Reviewer's comment ID #: 256-3)]	Accepted. Text modified.
9-401	A	23:22	23:22	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1097)]	Rejected. Simple style preference.
9-402	A	23:23	23:23	Insert after "without" "It is, however, very difficult to simulate the period from 1855 to 1910, with its slight cooling, followed with a steady rise from 1910 to 1940 with a combination of volcanic activity and solar changes" [VINCENT GRAY (Reviewer's comment ID #: 88-1098)]	Rejected. The reviewer's assertion is incorrect.
9-403	A	23:25	23:27	How do the authors distinguish natural internal variability from the variability caused by the solar and volcanic forcings in the models (for the natural only runs) and all forcings in the observations in Figure 9.4.1? Nothing can be said about natural internal variability and how well the models simulate it from the diagram alone.	Reference is made in the text to sectoin 9.4.1.3 where variability is compred quantitatively. We have also added the following at the beginning of this

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				[Gareth S. Jones (Reviewer's comment ID #: 121-89)]	sentence "The interannual variability in individual simulations that is evident in …" implying that the high frequency interannual variability is an indication of internal variability.
9-404	A	23:26	23:26	Replace "natural internal variability" with " a few of the climate influences other than in the atmosphere" [VINCENT GRAY (Reviewer's comment ID #: 88-1099)]	Rejected. Reviewer provides no justification for this change.
9-405	A	23:26	23:26	Delete "also" [VINCENT GRAY (Reviewer's comment ID #: 88-1101)]	Rejected. Reviewer provides no justification for this change. Simple style preference.
9-406	A	23:26	23:26	Insert after "well" "provided they can be assumed to change in a linear fashion" [VINCENT GRAY (Reviewer's comment ID #: 88-1102)]	Rejected. Reviewer provides no justification for this change. Don't understand his point.
9-407	A	23:28	23:28	Delete "quite well" [VINCENT GRAY (Reviewer's comment ID #: 88-1100)]	Rejected. Reviewer provides no justification for this change.
9-408	A	23:33	23:33	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1103)]	Rejected. Simple style preference.
9-409	A	23:34	23:34	Insert before "the observed" "In contrast to" [VINCENT GRAY (Reviewer's comment ID #: 88-1104)]	Rejected. Making the change sugested by the reviewer would make the passage nonsensical.
9-410	A	23:34	23:37	Although the text here refers to both Figure 9.4.2 upper left panel and Figure 3.2.9 of Ch03 as if they were essentially the same, the former is much more sparse in coverage. E.g. Fig 3.2.9 shows values for most of Africa, and Fig 9.4.2 (upper left) does not. The reader is being led to compare the two figures and will obviously want to know why the two chapters come up with different assessments as to what is/ is not a reliable trend at the grid cell level. [Martin Manning (Reviewer's comment ID #: 155-66)]	We now provide full details of criteria for missing data and other details of how the figures were produced in Appendix 9.C (supplementary information)..
9-411	A	23:35	23:35	Insert at the beginning "to the actual record, which is highly irregular and contains several sustained temperature falls, the falsely linearised" [VINCENT GRAY (Reviewer's comment ID #: 88-1105)]	Rejected. The non-linear temperature variations have been discussed earlier.
9-412	A	23:35	23:35	Insert after "shows" "bogus" [VINCENT GRAY (Reviewer's comment ID #: 88-1106)]	Rejected. The reviewer's implicit assertion is untrue.
9-413	A	23:35	23:35	Delete "and Chapter 3, Figure 3.2.9" as you have this in line 37. [David Parker (Reviewer's comment ID #: 195-130)]	Accepted. Text modified.
9-414	A	23:37	23:37	Insert before "Such" "Unsurprisingly"	Rejected. Simple style preference.

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				[VINCENT GRAY (Reviewer's comment ID #: 88-1107)]	
9-415	A	23:37	23:37	Replace "a pattern" with "such an artificially fabricated pattern" [VINCENT GRAY (Reviewer's comment ID #: 88-1108)]	Rejected. The pattern has not been "fabricated".
9-416	A	23:38	23:38	Replace "modes of climate variabilty" with "many climate influences, but it is capable of being related to the many changes in the number, reliabilty, location and surroundings of the measuring instruments" [VINCENT GRAY (Reviewer's comment ID #: 88-1109)]	Rejected. The reviewer's assertion is not correct and has been demonstrated to be incorrect previously.
9-417	A	23:38	23:38	Replace "For example" with "Also" [VINCENT GRAY (Reviewer's comment ID #: 88-1110)]	Rejected. Simpe style preference.
9-418	A	23:41	23:41	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1111)]	Rejected. Simple style preference.
9-419	A	23:42	23:42	Replace "observed" with "the false linear trend fitted to the observed record" [VINCENT GRAY (Reviewer's comment ID #: 88-1112)]	Rejected. The linear warming trend does not reveal all the variations in temperature, but the reality is that there has been warming over this period.
9-420	A	23:44	23:44	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1113)]	Rejected. Simple style preference.
9-421	A	23:45	23:45	Add at end "and even better agreement if the influence of urban and land-use changes are included" [VINCENT GRAY (Reviewer's comment ID #: 88-1114)]	Rejected. Reviewer provides no justification for this change.
9-422	A	23:49	23:49	Add at beginning "There is little evidence to show that" [VINCENT GRAY (Reviewer's comment ID #: 88-1115)]	Rejected. Reviewer provides no justification for this change.
9-423	A	23:49	23:57	You could save space by omitting the 2nd and 3rd sentences of this paragraph: the reasoning would still make sense and the text would flow better. [David Parker (Reviewer's comment ID #: 195-131)]	Partly accepted. 2nd sentence deleted, and text modified, 3rd sentence kept since it has useful information.
9-424	A	23:50	23:50	Insert after "forcings" "but" [VINCENT GRAY (Reviewer's comment ID #: 88-1116)]	Rejected. This change would make the sentence incomprehensible and silly.
9-425	A	23:50		why are anthropogenic effects listed as 'external' forcings - is that defined somewhere? [David Rind (Reviewer's comment ID #: 214-92)]	Rejected. External forcings are defined and discussed in section 9.1.1.
9-426	A	23:51	23:51	Replace "both" by "only" [VINCENT GRAY (Reviewer's comment ID #: 88-1117)]	Rejected. The reviewer seems to think there are forcings that are not either anthropogenic or non-anthropogenic. Don't know what these could be.
9-427	A	23:51	23:51	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1118)]	Rejected. Simple style preference/
9-428	A	23:51	23:51	Insert after "forcing" but does not include surface human influenmces such as urban and	Rejected. A point is that you don't need

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				land-use changes" [VINCENT GRAY (Reviewer's comment ID #: 88-1119)]	these extra forcings to reproduce the temperature variations and change.
9-429	A	23:51	23:51	Delete "successfully" [VINCENT GRAY (Reviewer's comment ID #: 88-1120)]	Rejected. Reviewer provides no justification for this change.
9-430	A	23:54	23:54	Replace "externaL" by "other climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1121)]	Rejected. Reviewer appears to want to say that the climate is affected by the climate.
9-431	A	23:54	23:54	Delete "external control" [VINCENT GRAY (Reviewer's comment ID #: 88-1122)]	Rejected. Reviewer provides no justification for this change.
9-432	A	23:55	23:55	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1123)]	Rejected. Simple style preference.
9-433	A	24:2	24:2	Replace "are consistent in showing" with "show" [VINCENT GRAY (Reviewer's comment ID #: 88-1124)]	Rejected. reviewer has missed the point that the various models reproduce the same thing here.
9-434	A	24:2	24:2	Insert before "global" falsely linearised" [VINCENT GRAY (Reviewer's comment ID #: 88-1125)]	Rejected. Reviewer provides no justification for this change.
9-435	A	24:2	24:2	Replace "observed" by "shown by the surface observations" [VINCENT GRAY (Reviewer's comment ID #: 88-1126)]	Rejected. Section is about surface temperature.
9-436	A	24:2	24:2	Delete "only be" [VINCENT GRAY (Reviewer's comment ID #: 88-1127)]	Rejected. Reviewer provides no justification for this change.
9-437	A	24:2	24:30	Consider moving these two paragraphs to follow page 23 line 28. [David Parker (Reviewer's comment ID #: 195-132)]	Accepted. Text moved.
9-438	A	24:3	24:3	Replace "cxaptured" by "simulated" [VINCENT GRAY (Reviewer's comment ID #: 88-1128)]	Agreed.
9-439	A	24:3	24:3	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1129)]	Rejected. Simple style preference.
9-440	A	24:3	24:3	Add at the end "although the use of local human-induced effects from urban and land-use change give a more likely explanation" [VINCENT GRAY (Reviewer's comment ID #: 88-1130)]	Rejected. Reviewer provides no justification for this change.
9-441	A	24:4	24:4	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1131)]	Rejected. Simple style preference.
9-442	A	24:7	24:7	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1132)]	Rejected. Simple style preference.
9-443	A	24:8	24:9	"much more likely" to "much more probably" [Daithi Stone (Reviewer's comment ID #: 256-4)]	Rejected. Prefer likely.

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9-444	A	24:9	24:9	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1133)]	Rejected. Simple style preference.
9-445	A	24:9	24:9	Insert after "warming" ."as indu\icated by the uncorrected surface record" [VINCENT GRAY (Reviewer's comment ID #: 88-1134)]	Rejected. there is no "warming" on this line.
9-446	A	24:9	24:9	Add at end "but human-induced changes on the surface such as urban and land-use changes are a much more likely explanation [VINCENT GRAY (Reviewer's comment ID #: 88-1135)]	Rejected. Reviewer provides no justification for this change.
9-447	A	24:11	24:11	Insert after "century "observed surface" [VINCENT GRAY (Reviewer's comment ID #: 88-1136)]	Rejected. Section is about surface temperature.
9-448	A	24:12	24:12	Insert after "warming" "but, of course, they ignore the obvious influences of instrunment numbers, location and reliability" [VINCENT GRAY (Reviewer's comment ID #: 88-1137)]	Rejected. This is not true.
9-449	A	24:12	24:14	most of these studies use a relatively large solar forcing component; that should be mentioned. [David Rind (Reviewer's comment ID #: 214-93)]	Accounted for, this is now mentioned in the chapter..
9-450	A	24:12	:14	Most of these studies use a relatively large solar forcing component; that should be mentioned. [Govt. of United States of America (Reviewer's comment ID #: 2023-581)]	Accepted. Text modified.
9-451	A	24:14	24:14	Add Stott 2003a reference to the solar forcing before 1950 part, as it also finds a significant role for solar before 1950. [Gareth S. Jones (Reviewer's comment ID #: 121-90)]	Accepted. Text modified.
9-452	A	24:27	24:28	Cite also here Mann and Park (1994): Mann, M.E., Park, J., Global scale modes of surface temperature variability on interannual to century time scales, Journal of Geophysical Research, 99, 25819-25833, 1994. [Michael Mann (Reviewer's comment ID #: 156-10)]	Accepted. Reference added.
9-453	A	24:30	24:30	Add at end "the universal disregard of the most important influences, such as instrument numbers, reliability, location, and the changes around them, mesns that most of these studies are of marginal significance" [VINCENT GRAY (Reviewer's comment ID #: 88-1138)]	Rejected. This is not true.
9-454	A	24:32	:41	The authors rightly note that the ability of climate models to reproduce observed temperature trends in the 20th century may be a function of improved models, or a fortuitous occurrence caused by compounding errors. This important point is now much better referenced. However, the implications to policy makers should be discussed in the summary. Should be noted in summary on p. 38, but this chapter is not discussing policy implications. [Govt. of United States of America (Reviewer's comment ID #: 2023-582)]	Accepted. Sentence added to 9.4.5 summary section.

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9-455	A	24:33	24:33	Replace "anthropogenic" with "human-induced" in both places [VINCENT GRAY (Reviewer's comment ID #: 88-1139)]	Rejected. Simple style preference.
9-456	A	24:34	24:33	Insert after "forcings "but ignore other human-induced effects, such as urban and land-use changes" [VINCENT GRAY (Reviewer's comment ID #: 88-1140)]	Rejected. The ability to simulate the temperature record without these influences suggests that they do not influence global surface temperature variations.
9-457	A	24:34	24:34	Replace "anthropogenic" with "human-induced" in both places [VINCENT GRAY (Reviewer's comment ID #: 88-1141)]	Rejected. Simple style preference.
9-458	A	24:34	24:34	Insert after "climate" "with the local changes in urban and land-use being far more important" [VINCENT GRAY (Reviewer's comment ID #: 88-1142)]	Rejecteded. Reviewer provides no justification for this change.
9-459	A	24:35	24:36	Replace "could have been" with "was" [VINCENT GRAY (Reviewer's comment ID #: 88-1143)]	Rejected. Reviewer provides no justification for this change.
9-460	A	24:36	24:36	Insert after "fortuitously" "not only because local human changes at the surface have been ignored, but also" [VINCENT GRAY (Reviewer's comment ID #: 88-1144)]	Rejected. See 9-456.
9-461	A	24:41	24:41	Add at end "but, of course, ignoring many other important factors" [VINCENT GRAY (Reviewer's comment ID #: 88-1145)]	Rejected. See 9-456.
9-462	A	24:47	24:48	the study is restricted to the direct effect only so it really needs to be specified. Moreover there are indications suggesting this may not hold for the indirect effect. [Govt. of France (Reviewer's comment ID #: 2010-61)]	Accepted. Text refers to Sexton et al which shows indications might not hold for indirect effect.
9-463	A	25:3	25:3	Delee "reasonably well" [VINCENT GRAY (Reviewer's comment ID #: 88-1146)]	Rejected. Reviewer provides no justification for this change.
9-464	A	25:4	25:4	Insert before "observations" "surface" [VINCENT GRAY (Reviewer's comment ID #: 88-1147)]	Rejected. Section is about surface record.
9-465	A	25:5	25:5	the diagnostics are not restricted to internal variability as transient simulations include all the known forcings for the instrumental period. Suggest replacing internal by "external and internal [Govt. of France (Reviewer's comment ID #: 2010-62)]	Accepted. "Internal" deleted from line 5.
9-466	A	25:5	25:5	Delete "internal" [VINCENT GRAY (Reviewer's comment ID #: 88-1148)]	Rejected. Reviewer provides no justification for this change.
9-467	A	25:10	25:10	Replace "anthropogenic" with "human-induced" in both places [VINCENT GRAY (Reviewer's comment ID #: 88-1149)]	Rejected. Simple style preference.
9-468	A	25:36	25:36	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1150)]	Rejected. See 9-467.

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9-469	A	25:38	25:38	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1151)]	Rejected. See 9-467.
9-470	A	25:38	25:38	Insert after "record" "and" [VINCENT GRAY (Reviewer's comment ID #: 88-1152)]	Rejected. Poor grammar.
9-471	A	25:38	25:38	Delete "and strengthening anthropogenic signal" [VINCENT GRAY (Reviewer's comment ID #: 88-1153)]	Rejected. Reviewer provides no justification for this change.
9-472	A	25:38	25:39	Delete "confidence in" [VINCENT GRAY (Reviewer's comment ID #: 88-1154)]	Rejected. Reviewer provides no justification for this change.
9-473	A	25:41	25:41	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1155)]	Rejected. See 9-467.
9-474	A	25:41	25:41	Insert after "warmin' "in the surface record" [VINCENT GRAY (Reviewer's comment ID #: 88-1156)]	Rejected. See 9-464.
9-475	A	25:41	25:42	Replace " is a result that is robust to" with "covers" [VINCENT GRAY (Reviewer's comment ID #: 88-1157)]	Rejected. Reviewer appears to have misunderstood this sentence.
9-476	A	25:42	25:45	I conceitedly suggest adding the following recently accepted papers: Stone, Allen, Selten, Kliphuis, Stott. 2006. The detection and attribution of climate change using an ensemble of opportunity. Journal of Climate; Stone, Allen, Stott. 2006. A multi-model update on the detection and attribution of global surface warming. Journal of Climate; Crooks, Allen, Stott. 2006. An update on the detection and attribution of observed temperature change - a space-time separable approach. Journal of Climate [Daithi Stone (Reviewer's comment ID #: 256-5)]	The papers that are in press have been included.
9-477	A	25:45	25:45	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1158)]	Rejected. See 9-467.
9-478	A	25:45	25:52	Repeating comments made in response to the FOD: Fomby and Vogelsang do not do any attribution, so their paper should not be cited in this paragraph. Kaufmann and Stern's conclusions were refuted by Umberto Triacca, "On the use of Granger Causality to investigate the human influence on climate." Theor. Appl. Climatol. 69, 137-138 (2001). He showed that the structure of their VAR analysis could not, in principle, discriminate between the conclusion of human influence on climate and the conclusion of no human influence on climate. Their estimated results are consistent with either conclusion. [Ross McKitrick (Reviewer's comment ID #: 174-36)]	Accepted. Text modified.
9-479	A	25:46	25:46	Add at begnning "global surface mean temperature" [VINCENT GRAY (Reviewer's comment ID #: 88-1159)]	Rejected. See 9-467.
9-480	A	25:46	25:46	Replace "also increase confidence" with "were also made" [VINCENT GRAY (Reviewer's comment ID #: 88-1160)]	Rejected. Reviewer provides no justification for this change.
9-481	A	25:47	25:47	Delete "robust"	Rejected. Reviewer provides no

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				[VINCENT GRAY (Reviewer's comment ID #: 88-1161)]	justification for this change.
9-482	A	25:47	25:47	Insert after "mean" "global surface" [VINCENT GRAY (Reviewer's comment ID #: 88-1162)]	Rejected. See 9-467.
9-483	A	25:48	25:48	Replace "natural climate varianility" with "all other climate onfluences except gre\nhouse gases etc." [VINCENT GRAY (Reviewer's comment ID #: 88-1163)]	Rejected. Reviewer mis-interprets this paper.
9-484	A	25:50	25:50	Insert after "temperatures" "indicatinmg an imporatnt contribution from uirbanisation and land-use changes" [VINCENT GRAY (Reviewer's comment ID #: 88-1164)]	Rejected. Reviewer's conclusion does not follow.
9-485	A	25:51	25:51	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1165)]	Rejected. See 9-467.
9-486	A	25:51	25:51	Add at end "and human effects from urbanisaytion and land-use change" [VINCENT GRAY (Reviewer's comment ID #: 88-1166)]	Rejected. Reviewer provides no justification for this change.
9-487	A	25:55	25:55	Insert after "observed' "surface" [VINCENT GRAY (Reviewer's comment ID #: 88-1167)]	Rejected. Section is about surface.
9-488	A	25:55	25:55	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1168)]	Rejected. See 9-467.
9-489	A	25:56	25:56	Insert after "aerosols" " and urban and land-use change": [VINCENT GRAY (Reviewer's comment ID #: 88-1169)]	Rejected. Reviewer apparently believes that urbanisation leads to cooling.
9-490	A	25:57	25:57	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1170)]	Rejected. See 9-467.
9-491	A	26:2	26:2	Replace "well-mixed" by "minor". No gases are "well-mixed" [VINCENT GRAY (Reviewer's comment ID #: 88-1171)]	Rejected. Standard usage.
9-492	A	26:3	26:3	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1172)]	Rejected. See 9-467.
9-493	A	26:5	26:5	Replacve "linear" with "pseudo-linear" [VINCENT GRAY (Reviewer's comment ID #: 88-1173)]	Rejected. The trend was estimated by fitting a linear trend to the data. Fitting other functions still reveals warming trend.
9-494	A	26:5	26:5	Delete "well-mixed" They are NOT "well-mixed" [VINCENT GRAY (Reviewer's comment ID #: 88-1174)]	Rejected. Standard usage.
9-495	A	26:6	26:6	Replace "0.9±0.24" with "0.9±0,48" to give 95% confidence figures [VINCENT GRAY (Reviewer's comment ID #: 88-1175)]	Rejected. These are 5 to 95 percentiles. Text clarifued
9-496	A	26:6	26:6	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1176)]	Rejected. See 9-467.

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9-497	A	26:6	26:6	Replace "0.4±0.26" with "0.4±0.52" to give 95% confidence figures [VINCENT GRAY (Reviewer's comment ID #: 88-1177)]	See 9-495.
9-498	A	26:7	26:7	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1178)]	Rejected. See 9-467.
9-499	A	26:7	26:7	Insert before "trend" "spurious" [VINCENT GRAY (Reviewer's comment ID #: 88-1179)]	Rejected. Trend is not spurious
9-500	A	26:7	26:7	Replace "0.5±0.15" with "0.5±0.30" to give 95% confidence figures [VINCENT GRAY (Reviewer's comment ID #: 88-1180)]	See 9-495.
9-501	A	26:7	26:7	Insert after "century" "the few" [VINCENT GRAY (Reviewer's comment ID #: 88-1181)]	Rejected. The ability of models to simulate the temperature variations indicates that any missing natural forcings have little impact.
9-502	A	26:8	26:8	Insert after "forcings "considered" [VINCENT GRAY (Reviewer's comment ID #: 88-1182)]	Rejected. See 9-501.
9-503	A	26:9	26:9	Delete "best" [VINCENT GRAY (Reviewer's comment ID #: 88-1183)]	Rejected. Reviewer provides no justification for this change.
9-504	A	26:11	26:11	Replace "internal variability" with "other factors, which include urban and land-use chnages" [VINCENT GRAY (Reviewer's comment ID #: 88-1184)]	Rejected. These are external forcings.
9-505	A	26:14	26:14	Add at end "but. Again. Insufficient attention was given to the influences of urban and land-use changes" [VINCENT GRAY (Reviewer's comment ID #: 88-1185)]	Rejected. Realistic simulations without these factors means that they are of little import in this context.
9-506	A	26:16	:57	Too wordy and involved. Be more succinct and pointed. [Govt. of United States of America (Reviewer's comment ID #: 2023-583)]	Accepted. Text revised.
9-507	A	26:17	26:18	Replace "internal variability" by "other climate factors, including urban and land-use changes" [VINCENT GRAY (Reviewer's comment ID #: 88-1186)]	Rejected. These are external forcings.
9-508	A	26:29	26:29	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1187)]	Rejected. Simple style preference.
9-509	A	26:34	26:34	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1188)]	Rejected. See 9-509.
9-510	A	26:38		the above paragraphs give a range of greenhouse warming for the 20th century of 0.6 to 1.5°C - how has this constrained their warming? [David Rind (Reviewer's comment ID #: 214-94)]	Rejected. The warming range due to grreenhouse gases is much less than forward estimates of the uncertainty in aerosol forcing would indicate.
9-511	A	26:38		The previous paragraphs give a range of greenhouse warming for the 20th century of 0.6	See 9-510.

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				to 1.5°C - how has this constrained their warming? [Govt. of United States of America (Reviewer's comment ID #: 2023-584)]	
9-512	A	26:48	26:51	This may be a good place to add Stone, Allen, Selten, Kliphuis, Stott (2006), and Stone, Allen, Stott (2006), perhaps instead of at comment 5. [Daithi Stone (Reviewer's comment ID #: 256-6)]	Accepted.
9-513	A	26:54	26:55	This may be a good place to add Crooks, Allen, Stott (2006), perhaps instead of at comment 5. [Daithi Stone (Reviewer's comment ID #: 256-7)]	Rejected. Paper not yet in press.
9-514	A	26:55		The Crooks and Gray study followed an earlier one, which introduced this approach using NCEP data: Haigh, J D (2003) The effects of solar variability on the Earth's climate. Phil. Trans. Roy. Soc A., 361, 95-111. [Joanna Haigh (Reviewer's comment ID #: 95-12)]	Noted. Both papers now cited in Section 9.2.2.1.
9-515	A	27:16	27:16	Delete "robust" [VINCENT GRAY (Reviewer's comment ID #: 88-1189)]	Rejected. Reviewer provides no justification for this change. Including his suggested change would misrepresent the science.
9-516	A	27:18	27:18	Delete "it was likely that" [VINCENT GRAY (Reviewer's comment ID #: 88-1190)]	Rejected. See 9-515.
9-517	A	27:19	27:19	Replace "were" with "might be" [VINCENT GRAY (Reviewer's comment ID #: 88-1191)]	Rejected. "were" is already modified by "was likely" – standard IPCC uncertainty language.
9-518	A	27:20	27:20	Delete "it was likely that" [VINCENT GRAY (Reviewer's comment ID #: 88-1192)]	Rejected. This phrase is not located on line 20.
9-519	A	27:21	37:21	Replace "would" by "might" [VINCENT GRAY (Reviewer's comment ID #: 88-1193)]	Rejected. Reviewer provides no justification for this change.
9-520	A	27:25		it was noted earlier that the SH has warmed less than the NH; hence the use of the hemispheric temperature contrast by itself would imply aerosols were a positive forcing. Heat uptake by the ocean has to be taken into account here. [David Rind (Reviewer's comment ID #: 214-95)]	Accepted. Text modified.
9-521	A	27:25		It was noted earlier that the SH has warmed less than the NH; hence the use of the hemispheric temperature contrast by itself would imply aerosols were a positive forcing. Heat uptake by the ocean has to be taken into account here. [Govt. of United States of America (Reviewer's comment ID #: 2023-585)]	See 9-520.
9-522	A	27:31	28:41	Too detailed. Is all this discussion, too often very technical and specialized, needed? [Govt. of United States of America (Reviewer's comment ID #: 2023-586)]	Accepted. The section has been shortened.
9-523	A	27:34	27:34	Insert after "Bayesian" "(or super-guesswork)"	Rejected. Bayesian analyses are not

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				[VINCENT GRAY (Reviewer's comment ID #: 88-1194)]	guesswork.
9-524	A	27:39	27:39	Insret before "Calibrated" "Bogus" [VINCENT GRAY (Reviewer's comment ID #: 88-1195)]	Rejected. Reviewer does not indicate why the statisticians Kass and Raftery's descriptors are bogus. His argument is clearly with them, rather than the authors of this chapter.
9-525	A	27:43	27:46	and 28:9-16: don't these paragraphs basically make the same point? [David Rind (Reviewer's comment ID #: 214-96)]	Accepted. Text has been shortened.
9-526	A	27:43	:46	Doesn't this para and that of page 28, lines 9-16, basically make the same point? [Govt. of United States of America (Reviewer's comment ID #: 2023-587)]	See 9-526.
9-527	A	27:46	27:46	Replace "internal variability" by "extraneous climate factors",,,three times [VINCENT GRAY (Reviewer's comment ID #: 88-1196)]	Rejected. What are 'extraneous climate factors"?
9-528	A	27:54	27:54	Delete "clear" [VINCENT GRAY (Reviewer's comment ID #: 88-1197)]	Rejected. Reviewer provides no justification for this change.
9-529	A	28:1	28:57	a lot of this material could be condensed and simply provided in summary statements. [David Rind (Reviewer's comment ID #: 214-97)]	Text has been shorteed
9-530	A	28:1	28:7	Add to here: Min, Hense. 2006. A Bayesian approach to climate model evaluation and multi-model averaging with an application to global mean surface temperature from IPCC AR4 coupled climate models. Geophysical Research Letters. [Daithi Stone (Reviewer's comment ID #: 256-8)]	Accepted. Reference added.
9-531	A	28:1	:57	Could this be made into a separate box, and just the summary statements provided here? Or perhaps use here more of what is on p. 96 and move the complexity to the appendix. [Govt. of United States of America (Reviewer's comment ID #: 2023-588)]	Text has been shorteed with references to the appendix material.
9-532	A	28:2	28:2	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1198)]	Rejected. Simple style preference.
9-533	A	28:3	28:3	Delete "decisive" [VINCENT GRAY (Reviewer's comment ID #: 88-1199)]	Rejected. Reviewer provides no justification for this change.
9-534	A	28:5	28:5	Replace "internal variability" with "extraneous climate factors" [VINCENT GRAY (Reviewer's comment ID #: 88-1200)]	Rejected. Reviewer provides no justification for this change.
9-535	A	28:7	28:7	Add at end "Of course, none of these studies considiered the possible influrence of urban or land-use changes on the surfaceand their influence on the instruments" [VINCENT GRAY (Reviewer's comment ID #: 88-1201)]	Rejected. Text already indicates what factors are taken into account..
9-536	A	28:19	28:19	Replave "internal variability" with "climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1210)]	Rejected. This phrase is not at this location.
9-537	A	28:24	28:24	Replave "internal variability" with "climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1211)]	Rejected. See 9-536.

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9-538	A	28:27	28:27	Delete"positive" [VINCENT GRAY (Reviewer's comment ID #: 88-1202)]	Agreed.
9-539	A	28:31	28:31	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1203)]	Rejected. See 9-532.
9-540	A	28:47	28:47	Replace "warming" with "temperature change" [VINCENT GRAY (Reviewer's comment ID #: 88-1204)]	Rejected. This change seems to be irrelevant to the argument here.
9-541	A	28:48		but unlike weather forecasting, the model errors are likely also the result of uncertainties in the forcing, hence predictions assuming they are only model errors are unlikely to be realized. This affects the "2.8°C max value assumed for the transient response at the time of 2CO2 doubling. [David Rind (Reviewer's comment ID #: 214-98)]	See 9-542.
9-542	A	28:48		Unlike weather forecasting, the model errors are likely also the result of uncertainties in the forcing, hence predictions assuming they are only model errors are unlikely to be realized. This affects the 2.8°C max value assumed for the transient response at the time of 2CO2 doubling. In addition, there are many different data opportunities for weather forecasting; using too limited a data set can produce worse (MOS) results. These caveats might lessen the strength of certain sentences in this paragraph. [Govt. of United States of America (Reviewer's comment ID #: 2023-589)]	Rejected. By having separate scaling factors on the response to greenhouse gases and other anthropogenic factors (dominated by aerosol forcing uncertainty) this analysis takes account of uncertainties in forcing. Text has benn clarified to make this point.
9-543	A	28:53	28:53	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1205)]	Rejected. See 9-532.
9-544	A	28:57	28:57	Delete "forecast" [VINCENT GRAY (Reviewer's comment ID #: 88-1206)]	Rejected. Reviewer provides no justification for this change.
9-545	A	29:5	29:5	Insert before "1%" "highly improbable" [VINCENT GRAY (Reviewer's comment ID #: 88-1207)]	Rejected. Reviewer provides no justification for this change.
9-546	A	29:7		Stone, Allen, Selten, Kliphuis, Stott (2006), and Stone, Allen, Stott (2006) also do this. [Daithi Stone (Reviewer's comment ID #: 256-9)]	Accepted. Reference included.
9-547	A	29:11	29:20	of course this assumes model accuracy in the latitudinal structure, etc of the warming (and models have quite different latitudinal structures of warming for the next century). [David Rind (Reviewer's comment ID #: 214-99)]	See 9-548
9-548	A	29:11	:20	Of course this assumes model accuracy in the latitudinal structure, etc of the warming (and models have quite different latitudinal structures of warming for the next century). Also, maybe natural and anthropogenic forcings could have similar regional/latitudinal structures. [Govt. of United States of America (Reviewer's comment ID #: 2023-590)]	Noted. In fact, the Braganza indinces include a measure of NH meridional temperature gradient and this has been noted in the text.
9-549	A	29:17	29:17	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1208)]	Rejected. Simple style preference.

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9-550	A	29:18	29:18	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1209)]	Rejected. See 9-549.
9-551	A	29:32		chapter 3 also notes that the 'observed' increase in cloud cover from surface observers contradicts the conclusion from satellites. [David Rind (Reviewer's comment ID #: 214-100)]	Noted. Text refers reader to chapter 3.
9-552	A	29:32		Chapter 3 also notes that the 'observed' increase in cloud cover from surface observers contradicts the conclusion from satellites. [Govt. of United States of America (Reviewer's comment ID #: 2023-591)]	See 9-551.
9-553	A	29:36	:56	Too many "could haves…." and far too convoluted of a discussion. Get to the point…..succinctly [Govt. of United States of America (Reviewer's comment ID #: 2023-592)]	Noted. Text rewritten to address these concerns.
9-554	A	29:53	29:53	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1212)]	Rejected. See 9-549.
9-555	A	30:1	:42	The focus on response to individual forcings is given far too much emphasis. One looses a sense of an order of importance here. The lengthy discussion of isolated effects of individual forcings leads to a "losing the of rest among the trees" syndrome. The main emphasis should be upon giving a clear message of net anthropogenic effect. Perhaps one should add, at the beginning of this page, a comment concerning the estimated net anthropogenic forcing before going into the details, so as to provide perspective. [Govt. of United States of America (Reviewer's comment ID #: 2023-593)]	Noted. The text has been ammended to address these concerns.
9-556	A	30:19	30:29	It might be worth adding to this description of different models application of forcing, that some models include volcanic effects by simply perturbing the incoming TOA shortwave, whilst others (most?) model it by simulating the radiative effects of the aerosols in the stratosphere. The difference causes differences in stratospheric temperature and subsequent long wave forcing. [Gareth S. Jones (Reviewer's comment ID #: 121-91)]	Accepted. Comment included.
9-557	A	30:38	30:39	Stone, Allen, Stott (2006) and Crooks, Allen, Stott (2006) also sample multi-model ensembles. [Daithi Stone (Reviewer's comment ID #: 256-10)]	This sentence has been deleted from revised text.
9-558	A	30:44	30:44	Replace "internal variability" by miscellaneous climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1220)]	Rejected. Uncertainties due to other, non-internal sources of variability are discussed elsewhere.
9-559	A	30:46	30:46	Replace "internal variability" by miscellaneous climate influence" [VINCENT GRAY (Reviewer's comment ID #: 88-1221)]	Rejected. See 9-558.
9-560	A	30:47	30:47	Replace "variability by large factors" by "other factors by large amounts" [VINCENT GRAY (Reviewer's comment ID #: 88-1222)]	Rejected. Reviewer appears to have mis-read this sentence.

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9-561	A	31:6	31:6	Replacxe "negligible" by "important" Gray (2000, "The Cause of Global Warming" Energy and Environment Volume 11, pages 613-639) has shown why it is not negligible and McKitrick & Michaels (2004 "A test of corrections for extraneous signals in gridded surface temperature data", Climate Research Vol 20 pages 159-173)) made a statistical study on individual stations and on the supposedly "corrected" mean gloibal temperature record from 1979 to 2000 and found a significant influence of a range of socioeconomic factors such as population, energy usage and prosperity [VINCENT GRAY (Reviewer's comment ID #: 88-1223)]	Rejected. See Chapter 3 and previous IPCC Assessments.
9-562	A	31:10	33:37	The section is important but at present it reads more like a review than an assessment. A fair summary of the present text would seem to this reader to be that the ability to attribute changes probably varies from region to region (e.g., Australia versus SE United States), and that there is increasing uncertainty in the confidence of models to simulate variability at smaller scales (whether or not the models happen to appear to match the variability level since this doesn't prove attribution of variability). Low frequency variability - e.g., that due to the NAO, PDO, or other slow changes in the ocean that influence various regions may or may not be well captured in models - and is also a confounding factor at regional levels, and I suggest that this needs to be stated. Finally, the issue of how much time and space averaging is needed for strong detection studies may be best called a topic for further research if aiming to cover the entire globe and this should be stated. If the above summary points aren't correct, then please add appropriate text to clarify them - but please ensure that this section end up with a clear synthesis and summary of the status of regional attribution - which it doesn't presently contain. [Susan Solomon (co-chair WG1) (Reviewer's comment ID #: 246-8)]	Agreed. Section 9.4.2 substantially redrafted to focus more clearly on the question of scales. Note that the title has also changed. Specifically mention possible confounding effects of NAO etc on "regional" scale detection. Assessment is located in 9.4.5 (as is the case for all sections in 9.4 – body text is a review and final assessment is in 9.4.5). Have included assessment sentences in specific paragraphs in 9.4.2. Revised assessment paragragh in 9.4.5 (which also now has been inserted in the ES) in 9.4.5 says that difficulties remain in simulating observed temperature changes in some parts of the the world and that averaging over smaller regions reduces the natural variability less than averaging over large regions making it more difficult to distinguish changes expected from external forcing.
9-563	A	31:15	31:15	Insert after "has been" an irregular series of cooling and warming sequences which do not justify the pretence of a linear behaviour, however if this pretence is made it is possible to claim" [VINCENT GRAY (Reviewer's comment ID #: 88-1224)]	Rejected. No matter how you fit the data there has been warming.
9-564	A	31:17	31:17	Insert after "Asia" "(enhanced by incomplete data (see McKitrick & Michaels (2004 "A test of corrections for extraneous signals in gridded surface temperature data", Climate Research Vol 20 pages 159-173)	Rejected. Warming evident even if this possible factor is considered.

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				[VINCENT GRAY (Reviewer's comment ID #: 88-1225)]	
9-565	A	31:18	31:19	Here it is stated "Since 1979, all areas of land show warming except for a small region of South America" implying no cooling over Antarctica. See previous comments #64 and #73 about SAM trends, and comment #133 below. [Adrian Simmons (Reviewer's comment ID #: 242-131)]	Accepted. Txt changed to read "almost" all land areas with observational data coverage."
9-566	A	31:30	31:30	Delete "do not" [VINCENT GRAY (Reviewer's comment ID #: 88-1226)]	Rejected. Reviewer provides no justification for this change.
9-567	A	31:30	31:30	Replace "natural internal variability" .by "other important climate influences, becaues they ignore El Niño ocean effects, urban and land-use changes, as well as instrument inaccuracies" [VINCENT GRAY (Reviewer's comment ID #: 88-1227)]	Rejected. Models do not underestimate variability even if they don't include urbanisation effects, arguing against its importance.
9-568	A	31:30	:34	Too much detail. Simplify to make the main point. Also the phrase 'natural internal variability' appears. See SPM comment regarding this usage. [Govt. of United States of America (Reviewer's comment ID #: 2023-594)]	Accepted. Text simplified.
9-569	A	31:31	31:31	Delete "although" and capitalise The" [VINCENT GRAY (Reviewer's comment ID #: 88-1228)]	Rejected. Simple style preference.
9-570	A	31:32	31:32	Replace "variability" with "changes in climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1229)]	Rejected. Focus here is on internal variability.
9-571	A	31:32	31:32	Replace "may not be well" by "are not" [VINCENT GRAY (Reviewer's comment ID #: 88-1230)]	Specific change rejected. However, text revised.
9-572	A	31:34	31:35	Don't really understand this sentence and in particular the use of the word "predictable". This is related to the question of the relevant spatial scales for detection studies which could be elaborated a little bit more in the text [Govt. of France (Reviewer's comment ID #: 2010-63)]	Accepted. The sentence has been replaced
9-573	A	31:34	31:34	Replace "can" by "mistakenly thought" [VINCENT GRAY (Reviewer's comment ID #: 88-1231)]	Rejected. Change is non-grammatical.
9-574	A	31:36	31:36	Replace "robustly" with "mistakenly" [VINCENT GRAY (Reviewer's comment ID #: 88-1232)]	Rejected. Reviewer provides no justification for this change.
9-575	A	31:37	31:37	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1233)]	Rejected. Simple style preference.
9-576	A	31:37	31:37	Insert after "this" "coubtful" [VINCENT GRAY (Reviewer's comment ID #: 88-1234)]	Rejected. Reviewer provides no justification for this change.
9-577	A	31:45	31:45	Replace "anthropogenic climate change" by "human induced changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1235)]	Rejected. Simple style preference.
9-578	A	31:49	31:49	Add at end "These studies ignore the fact that no such warming has been found in the lower troposphere"	Rejected. Reviewers' comment is incorrect.

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				[VINCENT GRAY (Reviewer's comment ID #: 88-1236)]	
9-579	A	32:1	32:1	is robust to" with "requires" [VINCENT GRAY (Reviewer's comment ID #: 88-1237)]	Rejected. Reviewer has appeared to mis-read the sentence.
9-580	A	32:2	32:2	Replace " [VINCENT GRAY (Reviewer's comment ID #: 88-1238)]	Comment incomplete, no response possible.
9-581	A	32:3	32:3	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1239)]	Rejected. See 9-575.
9-582	A	32:8	32:8	Replace "climate change" with "change in climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1240)]	Rejected. Simple style preference.
9-583	A	32:10	32:10	Add at end "which should be done anyway, as it is standard scientifc practice" [VINCENT GRAY (Reviewer's comment ID #: 88-1241)]	Rejected. No this is NOT standard scientific procedure. Reviewer has got things confused here.
9-584	A	32:14	32:14	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1242)]	Rejected. See 9-575.
9-585	A	32:16	32:16	Insert before "warming" "supposed" [VINCENT GRAY (Reviewer's comment ID #: 88-1243)]	Rejected. Reviewer provides no justification for this change.
9-586	A	32:22	32:22	After "(Knutson et al. 2005)", ADD the following statement "including the cooling trend downstream of the Tibetan Plateau (Zhou and Yu, 2006)". For the reference, see: "Zhou T., and R. Yu, 2006, 20th century surface air temperature over China and the globe simulated by coupled climate models, Journal of Climate, in press". I want to note that although there are serious debate on the impact of aerosol on the climate over Asia, according to Zhou and Yu (2006), among the nearly 20 IPCC AR4 models, only the GFDL model successfully reproduced the observational cooling downstream of the Tibetan Plateau in the 20th century climate simulation (20C3M). [Tianjun Zhou (Reviewer's comment ID #: 303-1)]	Rejected. Point is too specific for general assessment of continental and sub-continental scale detection results.
9-587	A	32:23	32:23	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1244)]	Rejected. See 9-575.
9-588	A	32:28	:34	Where a key point is being made, start the paragraph with it. So, in this para, begin with "Evidence now exists for a likely human influence on regional climates", rather than having that key statement be a trailer. There are other examples, in this Ch 9, of such poor paragraph structure. [Govt. of United States of America (Reviewer's comment ID #: 2023-595)]	Taken into account. This section has been ammended to to make a better and clearer assessment
9-589	A	32:34	32:34	Insert after "influence" " from urban and land-use changes" [VINCENT GRAY (Reviewer's comment ID #: 88-1245)]	Rejected. The reviewer's suggestion would mis-represent the science and the studies cited here.
9-590	A	32:36	32:36	Please consider carefully how the natural and anthropogenic forcing cases are normalized,	Accepted. Figures have been redrafted

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				and describe how it was done in the text. Also consider the differences in variability between the two cases here and in question 9.2., figure 1, and explain that for the reader. [Susan Solomon (co-chair WG1) (Reviewer's comment ID #: 246-14)]	and detailed description provided of how they were produced.
9-591	A	32:36	32:36	Please explain for the reader how to understand the apparently high confidence in detection in certain regions where there is very little data over the full 20th century as shown in figure 9.4.2. How is it that you can divide the globe so finely when you have only a few data points in some of these regions over the full 20th century? It seems surprising that you can have four meaningful subregions in Africa where your plot suggests a strong ability to attribute change, when you have so few observations there spanning the 20th century, for example. Please explain or change this and add appropriate qualifiers if needed; it's really important. [Susan Solomon (co-chair WG1) (Reviewer's comment ID #: 246-15)]	Accepted. Figures have been redrafted with a prescribed and documented coverage criterion (see Appendix 9.C, supplementary information). Deailed information on the firgure construction is now provied in the suplementary information.
9-592	A	32:38	:40	Be more concise. This lengthy sentence better broken into two. [Govt. of United States of America (Reviewer's comment ID #: 2023-596)]	Accepted. Paragraph redrafted to make clearer assessment.
9-593	A	32:39	32:40	Replace "natural internal variability" by "a whole range of climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1246)]	Rejected. It is important to distinguish between natural internal and natural external factors here.
9-594	A	32:41	:42	Here, and elsewhere in Ch 9, it becomes distracting when particular models (HadCM3, GFDL CM2, etc) are singled out. Is this always, or at all, necessary? No reason for these particular models to be discussed, positively or negatively - better to discuss the general model results. [Govt. of United States of America (Reviewer's comment ID #: 2023-597)]	Partly accepted. Use of model name has been reconsidered. However, often they are retained to help the clarify results, or where similar studies have not been performed with a representative selection of models.
9-595	A	32:46	32:46	Replace "internal variability" with "miscellaneous climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1247)]	Rejected. See 9-593.
9-596	A	32:52	33:13	Too much detail. It would suffice to state that optimal detection methods have been applied to subcontinental scales, without detailing the results of each one. De-emphasize the detail on the techniques. [Govt. of United States of America (Reviewer's comment ID #: 2023-598)]	Accepted. This paragraph deleted with details of some sub-continental scale studies provided in more concise form elsewhere where it helps make assessment.
9-597	A	32:57	32:57	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1248)]	Rejected. Simple style preference.
9-598	A	33:1	33:1	"G and GS" ? [Pascale DELECLUSE (Reviewer's comment ID #: 58-53)]	Accepted. Reference now made to anthropogenic influence in shorter paragraph.
9-599	A	33:3	33:3	Replace "anthropogenic" with "human-induced"	Rejected. Simpe style preference.

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				[VINCENT GRAY (Reviewer's comment ID #: 88-1249)]	
9-600	A	33:5		This reference to Min and Hense (2006) should instead be to the part 2 paper (the part 1 paper is referenced). [Daithi Stone (Reviewer's comment ID #: 256-11)]	Accepted. Thank you.
9-601	A	33:7	33:7	correct "claisified" [Pascale DELECLUSE (Reviewer's comment ID #: 58-54)]	Accepted. Corrected.
9-602	A	33:7	33:7	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1250)]	Rejected. See 9-599.
9-603	A	33:10	33:12	Whilst undoubtedly useful, I don't think atmosphere only models can enable the detection of weaker signals. In these type of studies the SSTs act like a forcing so will be a major contributor to the atmosphere response. It may then be possible to deduce the impact of another external forcing on the atmosphere temperatures, but the component from the SSTs that may also have been forced will not be detected. So depending on how much the SSTs may have responded to the forcing the result could actually produce a lower SNR pattern. [Gareth S. Jones (Reviewer's comment ID #: 121-92)]	Noted. Revised paragraph does discuss the point that the SSTs have not been explained.
9-604	A	33:11	33:1	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1251)]	Rejected. See 9-599.
9-605	A	33:15	:38	Again, too much review detail. There is ample room for shortening. [Govt. of United States of America (Reviewer's comment ID #: 2023-599)]	Accpeted. This section has been shortened.
9-606	A	33:32	33:32	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1252)]	Rejected. See 9-599.
9-607	A	34:20	34:30	correct "observataions" [Pascale DELECLUSE (Reviewer's comment ID #: 58-55)]	Agreed
9-608	A	34:25	:35	This paragraph is anything but clear. A re-write is needed. A figure exists in the technical summary (TS 3.4, Fig. 1) showing an idealized pdf of changes in temperature extremes that could be referred to (or included) here. Also, instead of saying 'underlying deterministic changes, one could just say 'mean' changes. [Govt. of United States of America (Reviewer's comment ID #: 2023-600)]	Accepted. Section has been rewritten and shortened.
9-609	A	34:37	35:37	This is an important topic but the discussion is too long and overlaps with Chapters 3 and 11. Most if not all of the text between P9-34, L55 and P9-35, L20 could be deleted. [Govt. of Finland (Reviewer's comment ID #: 2009-110)]	Accepted. Section has been rewritten and shortened.
9-610	A	34:52	34:54	Delete the sentence citing Stott et al. 2004 as this work isdescribed in full on page 35. [David Parker (Reviewer's comment ID #: 195-133)]	Accepted. Section has been rewritten and shortened.
9-611	A	34:53	34:53	note that CHP3 Box 3.5 is on tropical cyclones, and not summer 2003 in Europe [Pascale DELECLUSE (Reviewer's comment ID #: 58-56)]	Accepted. Although this particular sentence has been deleted. Correct

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					reference to box later in section.
9-612	A	34:55	34:55	Another related result has been published in Nature : Chuine I., P. Yiou, N. Viovy, B. Seguin, V. Daux et E. Le Roy Ladurie. Grape Harvest Dates and Temperature Variations in France since 1370, Nature, 289-290 (2004) [Pascale DELECLUSE (Reviewer's comment ID #: 58-57)]	Noted. We do not include this reference in shortened section as does not aid assemssment of attribution of European heatwave.
9-613	A	35:10	35:11	Tthere are also climate-related aspects of the 2003 heat wave which are relevant to complete the statistical detection studies by the suggestion of physical mechanisms. One such study is the following: Cassou C., L. Terray and A. S. Phillips, 2005: Tropical Atlantic influence on European Heatwaves J.Climate, 18, 2805-2811. This particular study shows that the 2003 summer is characterized by large anomalies in the occurrence frequency of specific weather regimes associated to warm conditions over western Europe. Moreover, it suggests that atmospheric teleconnections associated with latitudinal shifts of the Atlantic ITCZ may be responsible for the observed regime occurrence deviations. Suggest to add the Cassou et al. study to the Fink and Black references. [Govt. of France (Reviewer's comment ID #: 2010-64)]	Rejeceted. Paper does not aid assessment of whether external forcings or internal variability contributed to increase risk of European heatwave conditions. Paper is more directed at seasonal forecasting of Eropean summers. Also have delted Fink and Black reference in order to shorten section and avoid overlaps with chapter 3 (see comment 609)
9-614	A	35:13	35:13	Is the uniqueness of the signal to a particular forcing a important problem for sub-global attribution studies? I did not see it mentioned. [Ronald J Stouffer (Reviewer's comment ID #: 258-22)]	Noted. The issue of confounding signals at the regional scale is now discussed.
9-615	A	35:15	:18	Similar evidence exists from the analysis of variability on seasonal time scales. References include: Zwiers et al. 2000, JGR, 105, 7295-7315; Kumar et al. 2000, Journal of Climate, 13, 3139-3151. Also note that previous work, quoted in previous IPCC reports, have discussed reduced temperature variability as the latitudinal gradient decreases. The lack of change in variability is not a general result for all time--scales and all regions. [Govt. of United States of America (Reviewer's comment ID #: 2023-601)]	Noted. This sentence deleted in shortened section.2nd para in new section discusses literature on contribution of changes in variability to European summer extreme temperatures.
9-616	A	35:18	35:18	Clarify "as a two stage problem": e.g., "in terms of a change of mean only"? [David Parker (Reviewer's comment ID #: 195-134)]	Noted. Sentence has been deleted from section.
9-617	A	35:32		Please give a year for the red line – i.e. by when will the risk have reached this level? Also there is too much repetition between the figure caption and the text. The caption should just describe the variables shown and different graphic elements of the figure and the main text should contain the interpretation. [Martin Manning (Reviewer's comment ID #: 155-67)]	Agreed. Included reference to when risk will have reached red curve level in text. Ammended figure caption.
9-618	A	35:34	35:37	it may be worth adding that the study has also considered mean temperature while observations show different amplitude and patterns for minimum and maximum temperature. Explicit consideration of these differences may also shed some light on the	Noted. There is no specific literature to cite here so no addition made to text.

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				associated physical processes and nicely complement the statistical detection study. [Govt. of France (Reviewer's comment ID #: 2010-65)]	
9-619	A	35:41	38:4	This whole section ignores the El Niño ocean effects which are so prominently seen in all the temperature records and which you categorise, wrongly, as an "internal variability". Presumably this is to draw attention away from the fact that these effects are being recruited as spurious human-induced forcing effects [VINCENT GRAY (Reviewer's comment ID #: 88-1272)]	Rejected. El Nino is an internal mechanism.
9-620	A	35:46	35:46	Replace (ince 1958) with "show no change in temperature between 1958 and 2004" [VINCENT GRAY (Reviewer's comment ID #: 88-1253)]	Rejected. See Chapter 3.
9-621	A	35:46	35:46	Replace "(since 1979)" with "show no temperature change from 1979 to 1998, but then was followed by a sharp 1999 peak attributed to the El Niño ocean effect, and a short cooling spell afterwards . These results show that the supposed human-induced forcing cannot be detrected in the region where it is supposed to happen, so all the studies with the surface record are worthless." [VINCENT GRAY (Reviewer's comment ID #: 88-1254)]	Rejected. Do not understand his point. Seems to agree that warming has bene observed but then says it hasn't been detected.
9-622	A	35:46	35:47	Replace "show warming trends in the troposphere and" with They show" [VINCENT GRAY (Reviewer's comment ID #: 88-1255)]	Rejected. Suggested change is ungrammatical and would make sentence meaningless.
9-623	A	35:47	35:47	Replace "All datasets show" by "The satellite record can be manipullated by pretending that the 1999 El Niño peak can be held responsible for a human-induced trend to" [VINCENT GRAY (Reviewer's comment ID #: 88-1256)]	Rejected. See Chapter 3..
9-624	A	35:47	35:50	these lines appear to contradict one another - is it or is it not clear whether the troposphere warmed more than the surface (and for the tropics as well)? [David Rind (Reviewer's comment ID #: 214-101)]	Dont think there is a contradiction here.Add trend to text to make it clear that we're talking about an overall trend which can be different over thelonger and shorter periods.
9-625	A	35:47	:50	If the data isn't good enough to conclude anything from 1979 to the present, how can we really conclude anything from 1958 to the present? [Govt. of United States of America (Reviewer's comment ID #: 2023-602)]	Rejected. Over longer periods there can be a smaller influence of error.
9-626	A	35:49	36:2	Delete all this section. It is disgraceful nonsense. The large El Niño peak on the stelliete record cannot be used to pretend that there was a "linear trend", or that this "trendf" could possibly be connected with "human-induced "forcing". The only "Trend" woth mentioning is the zero trend in the satellite record between 1979 and 1998 which show conclusively that human-induced "forcing is undetectable in the region where it is supposed to be prominent. [VINCENT GRAY (Reviewer's comment ID #: 88-1257)]	Rejected. See Chapter 3. Warming is evident even if 1998 is excluded

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9-627	A	35:49	36:2	I also object most strongly to the dismissal of the radiosonde record with wholly unjustified aspersions on its accuracy and validity. It records faithfully all the "natural" climate events (volcanos and El Niño) which are evident in the surface record as well as in the satellite record, which proves its reliabnility. What it does not do is record the supposed "warming" in the surface record, which casts doubt on that record, instead [VINCENT GRAY (Reviewer's comment ID #: 88-1258)]	Rejected. The aspersions on the radiosonde record are well-documented and supported and published in peer-reviewed journals.
9-628	A	36:4	36:26	The tropical and extratropical tropopause should be distinguished more clearly because different mechanisms control it. While the tropical tropopause clearly responds directly to changes in surface temperatures, the extratropical tropopause also responds to changes in surface temperature gradients. The interdecadal variations in extratropical tropopause height are not only due to recent near-surface warming, but changes in meridional surface temperature gradients in baroclinic zones contribute about equally to tropopause variations in the past 30 years (Schneider, T. The tropopause and the thermal stratification in the extratropics of a dry atmosphere. J. Atmos. Sci., 61, 1317-1340, 2004). [Michael Mann (Reviewer's comment ID #: 156-77)]	Partially rejected. We have insufficient space to describe controls on the tropopause in detail. However, we have inserted a sentence noting the possible influence of changes in surface temperature gradient, and citing the Schneider paper.
9-629	A	36:10	36:10	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1259)]	Rejected. Simple style preference.
9-630	A	36:18	36:18	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1260)]	Rejected. See 9-629.
9-631	A	36:26	36:36	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1261)]	Rejected. See 9-629.
9-632	A	36:31	:40	While the info in this paragraph is appreciated, this section should be initiated with a statement of the main point regarding the "robust detection of anthrop. Influences on the free atmosphere" [Govt. of United States of America (Reviewer's comment ID #: 2023-603)]	Accepted. A sentence has been added to the start of this paragraph.
9-633	A	36:34	36:34	Replace "temeperature" by "temperature" [VINCENT GRAY (Reviewer's comment ID #: 88-1262)]	Agreed.. Thanks.
9-634	A	36:34	36:34	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1263)]	Rejected. Simple style preference.
9-635	A	36:37	36:37	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1264)]	Rejected. See 9-634.
9-636	A	36:42	36:42	Delete "robust" [VINCENT GRAY (Reviewer's comment ID #: 88-1266)]	Rejected. "robust" seems a good description of a result that is widely reproduced.
9-637	A	36:42	36:42	Replace "found" by "claimed by the same illegitimate procedure of assuming that the	Rejected. Warming still evident if 1998

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				1999 El Niño temperature peak can generate a linear "trend related to human-induced forcing" [VINCENT GRAY (Reviewer's comment ID #: 88-1267)]	is excluded.
9-638	A	36:43	36:43	Versions of HadRT2 and not HadRT.1s [Peter Thorne (Reviewer's comment ID #: 264-16)]	Accepted. Thanks. Corrected.
9-639	A	36:45	36:45	Thorne et al., 2003 and not 2005a. [Peter Thorne (Reviewer's comment ID #: 264-17)]	Accepted. Thanks. Corrected.
9-640	A	36:51	36:51	Replace "robustly" with "spuriously" [VINCENT GRAY (Reviewer's comment ID #: 88-1268)]	Rejected. Reviewer provides no justification for this change.
9-641	A	36:55	36:55	Replace "robust" with "spurious" [VINCENT GRAY (Reviewer's comment ID #: 88-1269)]	Rejected. Reviewer provides no justification for this change.
9-642	A	37:7	37:7	Replace "spurious" with "alleged" [VINCENT GRAY (Reviewer's comment ID #: 88-1270)]	Rejected. Reviewer provides no justification for this change.
9-643	A	37:10	37:15	Whilst Crooks (2004) and Stott (2003a) suggest HadCM3 may be underestimating the response to solar at the surface and in the free atmosphere, Jones (2003) found that solar was only weakly detected but had scaling factors consistent with 1, i.e. the model neither under or overestimated the response, in a combined surface and free atmosphere analysis. This should be added here. [Gareth S. Jones (Reviewer's comment ID #: 121-93)]	Rejeted. Jones uses ols and shows a muich bigger amplitude for SOL when regressed against OBS (1.33) than when regress against ALL (0.13) indicating possible influence of low bias as expected from ols for weak signals. The other signals have much smaller differences between regression coefficients with ALL and with OBS.
9-644	A	37:17	:26	The role of observed SST changes in global terrestrial warming has not been adequately represented here. A recent EOS study, that appeared 9 May 2006, indicates the vast majority of terrestiral warmth observed in 2004, one of the warmest yrs in the instrumental record, could be explained as a response to the observed SST forcing. Result is overstated (due to the dominant influence of SSTs on a good portion of the tropospheric profile) and the lack of specifics about how the temperature profile really has been improved by addition of anthropogenic forcings. How much did ozone do, how much did CO2 do - and how well do we really know the vertical temperature profile change anyway, especially in the tropics. [Govt. of United States of America (Reviewer's comment ID #: 2023-604)]	Rejected. The aim here is to assess detection studies using observed SSTs not assess all lieterature using observed SSTs.
9-645	A	37:21	:22	Does the study cited based on a single model (from the reference it sounds like that)? There is other SST forced AGCM analysis where observed 200-mb heights are well reproduced (see Kumar et al. 2004, Journal of Climate, 17, 653-. Does the statement also imply that the direct influence of radiative forcing large? [Govt. of United States of America (Reviewer's comment ID #: 2023-605)]	Rejected. We assess the only study to have done a detection study with a model using observed SSTs. Reword to make clear.

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9-646	A	37:22	37:22	Replace "internal vartiability" with "all the other climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1271)]	Rejected. The proposed change would mis-represent the science and suggest greater confidence than actually exists.
9-647	A	37:31	37:36	again there appears to be a discrepancy here - in the tropics, is there too much tropospheric warming compared to the surface or not? [David Rind (Reviewer's comment ID #: 214-102)]	Accepted. Reworded to clarify.
9-648	A	37:31	:36	There appears to be a discrepancy here - in the tropics, is there too much tropospheric warming compared to the surface or not? [Govt. of United States of America (Reviewer's comment ID #: 2023-606)]	See 9-648.
9-649	A	37:52	37:53	The sentence about Vinnikov et al 2006 breaks the flow of thought. Maybe move down to line 57 and change the next sentence to begin "One possibility is that amplification…" [David Parker (Reviewer's comment ID #: 195-135)]	Accepted.
9-650	A	37:56	37:56	Change "is" to "was found to be". [David Parker (Reviewer's comment ID #: 195-136)]	Accepted.
9-651	A	38:5		The summary is excellent. Suggest that it lead the section, rather than trail the section. This is true for all the major summaries in the Chapter 9. The detailed literature reviews and evidences can follow the summaries, for those interested (which, after all, will mostly be the technical experts, and not decisionmakers). Is this structure an IPCC requirement or are the chapters free to place their summary statements where they want? Giving the highlights ahead of time often makes the following reading more comprehensible. [Govt. of United States of America (Reviewer's comment ID #: 2023-607)]	Rejecteed. Structure kept consistent with summaries at end of sections.
9-652	A	38:7	38:7	Replace "near" by "on" [VINCENT GRAY (Reviewer's comment ID #: 88-1273)]	Rejected. Actually, "near" is correct. Don't have thermometers sitting on the ground.
9-653	A	38:8	38:8	Insert after "influence" "due to the proximity of measuring instruments to human activity" [VINCENT GRAY (Reviewer's comment ID #: 88-1274)]	Rejected. No evidence that this is true.
9-654	A	38:9	38:9	With the greatest in 1999 due to the El Niño event ogf that year" [VINCENT GRAY (Reviewer's comment ID #: 88-1275)]	Rejected. The warmest 10-years also contain several non El-Nino years.
9-655	A	38:11	38:11	Insert after "include" "many climate influences, but exclude" [VINCENT GRAY (Reviewer's comment ID #: 88-1276)]	Rejected. Clearly these other influences are small.
9-656	A	38:11	38:11	Replace "forcings" by "influences", [VINCENT GRAY (Reviewer's comment ID #: 88-1277)]	Rejected. Would mis-represent the science.
9-657	A	38:11	38:11	Insert after "climate " "such as the El Niño ocean events, and urban and land-use effects" [VINCENT GRAY (Reviewer's comment ID #: 88-1278)]	Rejected. Would mis-represent the science and studies.
9-658	A	38:11	38:11	Delete "system" [VINCENT GRAY (Reviewer's comment ID #: 88-1279)]	Rejected. Simple style preference.

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9-659	A	38:13	38:13	Insert before "strong" "not very" [VINCENT GRAY (Reviewer's comment ID #: 88-1280)]	Rejected. Reviwer comment not supported by studies and makes sentence very silly.
9-660	A	38:15	38:15	Replace "very likely" with "highly unlikely to have" [VINCENT GRAY (Reviewer's comment ID #: 88-1282)]	Rejected. Reviewer provides no justification for this change.
9-661	A	38:16	38:16	Replace "robust to" with independent of" [VINCENT GRAY (Reviewer's comment ID #: 88-1283)]	Rejected. Reviewer provides no justification for this change.
9-662	A	38:20	38:20	Insert after "simulations" "but this has depended on the false assumption that the prominent temperature peak in 1999 caused by the El Niño ocean change, was involved with human activities" [VINCENT GRAY (Reviewer's comment ID #: 88-1284)]	Rejected. Reviewer provides no justification for this change. Trend evident even when 1998 excluded.
9-663	A	38:21	38:21	Replace "adequate" with "ionadequate" [VINCENT GRAY (Reviewer's comment ID #: 88-1285)]	Rejected. Reviewer provides no justification for this change.
9-664	A	38:21	38:21	Replace "internal variability" with miscellaneous climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1286)]	Rejected. This would make the statement stronger than is justified.
9-665	A	38:23	38:23	Insert after "change" "on the surface" [VINCENT GRAY (Reviewer's comment ID #: 88-1287)]	Rejected. Warming also observed away from surface.
9-666	A	38:24	38:24	Add at end "because of the building of cities, energy emission and land-use change" [VINCENT GRAY (Reviewer's comment ID #: 88-1288)]	Rejected. Reviewer provides no justification for this change.
9-667	A	38:29		is the word 'likely' here used in the IPCC sense (hence, not 'very likely')? [David Rind (Reviewer's comment ID #: 214-103)]	See 9-668.
9-668	A	38:29		Is the word 'likely' here used in the IPCC sense (hence, not 'very likely')? [Govt. of United States of America (Reviewer's comment ID #: 2023-608)]	Yes.
9-669	A	38:40	:42	This sentence is obscure; better to add at the end of the previous sentence, 'partially offsetting greenhouse gas warming'. [Govt. of United States of America (Reviewer's comment ID #: 2023-609)]	Accepted. Text changed to be consistent with ES formulation.
9-670	A	38:44	38:44	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1289)]	Rejected. Simple style preference.
9-671	A	38:45	38:45	Insert after "areas" "as a result of urban development , energy consumptionm and land-use cahnge" [VINCENT GRAY (Reviewer's comment ID #: 88-1290)]	Rejected. Mis-represents the studies cited in this section.
9-672	A	38:48	38:48	Insert after "climate" "but not from greenhouse gases" [VINCENT GRAY (Reviewer's comment ID #: 88-1291)]	Rejected. See 9-671.
9-673	A	38:51	38:51	Insert after "regions" "because of globalised econmic development" [VINCENT GRAY (Reviewer's comment ID #: 88-1292)]	Rejected. Reviewer provides no justification for this change.

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9-674	A	38:51	38:52	Replace from "The cahnce that" on line 51 to "considering that " on line 52 with "although" [VINCENT GRAY (Reviewer's comment ID #: 88-1293)]	Rejected. Reviewer provides no justification for this change.
9-675	A	38:53	38:53	Replace "internal variability" with "other miscellaneous climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1294)]	Rejected. This would "double count" external natural forcings.
9-676	A	38:55	38:55	Delete from "Evidence to "is emerging" The sentence is unnecessary [VINCENT GRAY (Reviewer's comment ID #: 88-1295)]	Rejected. It is a good "topic" sentence.
9-677	A	38:55	38:55	Delete "significant" [VINCENT GRAY (Reviewer's comment ID #: 88-1296)]	Rejected. The trend is statistically significant.
9-678	A	38:57	38:57	Insert after "nights" "because of increases in comfort standards" [VINCENT GRAY (Reviewer's comment ID #: 88-1297)]	Rejected. Reviewer provides no justification for this change.
9-679	A	39:1	39:1	Insert ater "year" "for the same reason" [VINCENT GRAY (Reviewer's comment ID #: 88-1298)]	Rejected. See 9-678.
9-680	A	39:10	39:10	Replace "a significant" with "an unmeasureable" [VINCENT GRAY (Reviewer's comment ID #: 88-1299)]	Rejected. Reviewer provides no justification for this change. The change would produce a false statement.
9-681	A	39:10	39:10	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1300)]	Rejected. Style preference.
9-682	A	39:12	39:12	Insert after "has" "never" [VINCENT GRAY (Reviewer's comment ID #: 88-1301)]	Rejected. Reviewer provides no justification for this change.
9-683	A	39:12	39:12	Replace "as has the" with "but there may be an": [VINCENT GRAY (Reviewer's comment ID #: 88-1302)]	Rejected. Reviewer provides no justification for this change.
9-684	A	39:13	39:13	Delete "The combination of a warming troposphere and " [VINCENT GRAY (Reviewer's comment ID #: 88-1303)]	Rejected. Reviewer provides no justification for this change.
9-685	A	39:14	39:	Repolace "has likely" with could possibly have" [VINCENT GRAY (Reviewer's comment ID #: 88-1304)]	Rejected. Reviewer provides no justification for this change.
9-686	A	39:15	39:15	Replace "likely largely" by "possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-1305)]	Rejected. Reviewer provides no justification for this change.
9-687	A	39:19	39:19	Delete "some" [VINCENT GRAY (Reviewer's comment ID #: 88-1306)]	Rejected. Reviewer provides no justification for this change.
9-688	A	39:19	39:19	Delete "possible" [VINCENT GRAY (Reviewer's comment ID #: 88-1307)]	Rejected. These wont be discrepancies if there are errors in the data. So at the moment they are "possible".
9-689	A	39:21	39:21	Replace "are remarkably consistent in their predictions" with "predict"	Rejected. The important point is the

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				[VINCENT GRAY (Reviewer's comment ID #: 88-1308)]	consistency.
9-690	A	39:22	39:22	Delete "of" [VINCENT GRAY (Reviewer's comment ID #: 88-1309)]	Rejected. Reviewer provides no justification for this change.
9-691	A	39:23		this sentence appears meaningless. [David Rind (Reviewer's comment ID #: 214-104)]	Accepted. Text has been revised.
9-692	A	39:23		This sentence appears meaningless. [Govt. of United States of America (Reviewer's comment ID #: 2023-610)]	See 9-691
9-693	A	39:39	:41	No reason to begin this section of Ocean State Changes with "prognostic" language such as "warming of the surface should lead…..", "melting glaciers will….". Just state what the changes HAVE been, and then give the understanding of attributable causes. I suggest beginning the para with the sentence "The overall heat content…." [Govt. of United States of America (Reviewer's comment ID #: 2023-611)]	Rejected. Framing the discussion this way aids understanding of the reader and underscores that phiyicasl understanding underpins statistical attribution analyses.
9-694	A	39:41	39:41	Replace "climate change" with "changes in climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1310)]	Rejected. Style preference.
9-695	A	39:43	39:43	Replace "inceased by 14/1x10 to the 21 J" by "varied in a periodic fashion" [VINCENT GRAY (Reviewer's comment ID #: 88-1311)]	Rejected. Periodic would not correctly describe the change that has taken place.
9-696	A	39:43	39:43	Add "about" before 14.1x10**22J. This is a lot of uncertainty in this value. [Ronald J Stouffer (Reviewer's comment ID #: 258-23)]	Rejected. We use same form of words as Chap 5 ES.
9-697	A	39:44	39:44	Delete from "This overall" to "variations" [VINCENT GRAY (Reviewer's comment ID #: 88-1312)]	Rejected. Reviewer provides no justification for this change.
9-698	A	39:44	39:44	Replace "The fact that" by "Because" [VINCENT GRAY (Reviewer's comment ID #: 88-1313)]	Rejected. Change would make the sentence ungrammatical and silly.
9-699	A	39:45	39:45	Insert after is" "not" [VINCENT GRAY (Reviewer's comment ID #: 88-1314)]	Rejected. Reviewer provides no justification for this change.
9-700	A	39:46	39:47	Delete from "If the observed" on line 46 to "variability then" on line 47. The atmosphere is not warming, according to the radiosondes and satellites, unles you count the El Niño event of 1999 [VINCENT GRAY (Reviewer's comment ID #: 88-1315)]	Rejected. Not true, even if you exclude 1998.
9-701	A	39:47	39:47	Replace "have likely been" with "seem to be" [VINCENT GRAY (Reviewer's comment ID #: 88-1316)]	Rejected. Reviewer provides no justification for this change.
9-702	A	39:51	39:51	Replace "variability" with "periodicity" [VINCENT GRAY (Reviewer's comment ID #: 88-1317)]	Rejected. Reviewer provides no justification for this change.
9-703	A	39:55		The phrase "with evidence from …": what does that mean? [Fons Baede (Reviewer's comment ID #: 9-14)]	Accepted, text revised.

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9-704	A	40:1	40:2	"small revision downwards of the observed increase in heat cointent" seems awkward. Change to "a reduction in the heat content trend". [Ronald J Stouffer (Reviewer's comment ID #: 258-24)]	Accepted.
9-705	A	40:4	:25	This paragraph could be greatly reduced by eliminating details of each individual paper, and focus on the main message common across them. Provide main message, and then provide an occasional reference to back it up. [Govt. of United States of America (Reviewer's comment ID #: 2023-613)]	Partly accepted.. A summary sentence has been added to the start of the paragraph and the paragraphs restructured slightly to make flow of text clearer, but level of detail was found important to support assessment
9-706	A	40:10	40:10	Replace "natural internal variability" by "various climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1318)]	Rejected. This would mis-represent the cited study.
9-707	A	40:26	40:26	The authors might include mention of Hansen et al. (2005) (Earth's energy imbalance: confirmation and implications, Science, 308, 1431-1435). In their Fig. 2, Hansen et al. show a close match between the measured and calculated heat uptake by the ocean for the past decade, based on coupled climate model simulations using the observed changes is radiative forcing of the climate system. Their calculated 0.85 W/m2 global energy imbalance at TOA is the rate at which the oceans are warming at the present time, It is also a measure of the unrealized global warming that is already in the pipeline if the GHG increase were brought to a sudden halt. [Andrew Lacis (Reviewer's comment ID #: 138-14)]	Rejected. Hansen et al (2005) is discussed in section 9.7.
9-708	A	40:29		The phrases natural internal and natural external variability are used; in addition, the words 'observed signal' are used - the observations contain more than just 'signal'. [Govt. of United States of America (Reviewer's comment ID #: 2023-612)]	Noted. Text has been revised to improve clarity.
9-709	A	40:39	40:47	Not sure that models are really simulating oceanic heat uptake very well at least in the deeper levels: See Forest et al,GRL,2006 who estimate inversely that most models are significantly over-estimating heat uptake from combined atmospheric and ocean temperature records.. [Ronald Prinn (Reviewer's comment ID #: 202-8)]	See 9-710.
9-710	A	40:39	40:39	The statement that "the overall increase in ocean heat content is well explained by models" is simply not true. It is well known that there are tremendous differences in model simulations of heat uptake (Sokolov et al., 2003; Pierce et al., 2006) and thus most of the models cannot be right. The statement is also contradicted by the accompanying figure, fig. 9.5.1, which shows that the global mean heat-uptake below the mixed layer is being significantly overestimated by both the PCM and HadCM3 (see the figures on the extreme right). For the ensemble means it is overestimated by about a factor of 2 by the PCM and about 2 1/2 by the HadCM3. This is consistent with the results of Forest et al. (2006) that AOGCMs are generally overestimating how rapidly heat is being mixed below	Accepted. Sentence included.

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				the mixed layer. I recommend replacing this sentence by the following two sentences: "Although the heat uptake in the ocean cannot be explained without invoking anthropogenic forcing, there is evidence that the models are overestimating how rapidly heat is penetrating below the ocean's mixed layer (Forest et al., 2006; also see fig. 9.5.1)). In addition the decadal variability seen in Levitus et al (2000; 2005) (see Chapter 5, Section 5.2.2) is not well reproduced by models." [Peter Stone (Reviewer's comment ID #: 257-5)]	
9-711	A	40:39	40:47	There clearly remains a range of views on this question in the community. This is seen by contrasting this sentence with the para in Ch 5 (p 5-5, ll 34-35). I think the different views are now adequately represented across the report as a whole, though I think it would be prefererable to ackowledge the range of opinion explicitly in both chapters, e.g. by some text like: "While some studies have noted the potential importance of choice of infilling method in poorly sampled regions (Gregory et al. 2004, Achuta Rao et al. 2005), the consistency of the Levitus et al, Ishii et al and Willis et al. analyses adds confidence to their use in climate change studies". I know there has been much discussion of how to present this, and if what we have is the best that can be achieved, I think it is OK. [Comment also sent to Ch 5] [Richard Wood (Reviewer's comment ID #: 294-13)]	Accepted. Proposed text added.
9-712	A	40:39		Replace "explained" by "simulated" or "reproduced". Models do not "explain". [Fons Baede (Reviewer's comment ID #: 9-15)]	Accepted. Sentence replaced.
9-713	A	40:40	40:40	"well reproduced" I would delete "well". [Ronald J Stouffer (Reviewer's comment ID #: 258-25)]	Noted. Sentence replaced.
9-714	A	40:47	40:47	At the end of the sentence, add "particularly the Southern Ocean where models predict most of the oceanic heat uptake occurs". [Ronald J Stouffer (Reviewer's comment ID #: 258-26)]	Rejected. We make the point that it is the NH upper ocean that is well sampled when discussing Gregory et al.
9-715	A	40:55	40:55	"increase in meridional moisture fluxes" would be better than "global increase in the hydrological cycle" [Isaac Held (Reviewer's comment ID #: 105-40)]	Accepted. Text revised accordingly.
9-716	A	40:55	41:2	It is important to note that changes in ocean fresh water transport may also be a significant contributor. I believe there are a few papers 'in the works' which suggest that this is an important term. I think the text is just about defensible in a legalistic sense, but suggest adding a sentence "However, variations in other terms (e.g. ocean fresh water transport) may be contributing significantly to the observed salinity changes and have not been quantified." [Richard Wood (Reviewer's comment ID #: 294-14)]	Accepted.
9-717	A	40:57	41:1	why would one expect a reduction in P-E -- do you mean increase, which is what the models show?	Rejected. A reduction is what is seen in the observations according to 5.6 and is

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				[Isaac Held (Reviewer's comment ID #: 105-41)]	descrbied as being consistent with an increasein theEarth's hydrological cycle.
9-718	A	40:57		Strong statement about the increased in the global hydrologic cycle based on one reference. [Govt. of United States of America (Reviewer's comment ID #: 2023-614)]	Rejected. Statement supported by evidence in chapter 5 which is now referenced here.
9-719	A	41:17	41:17	Replace "climate change" with "changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1319)]	Rejected. Style preference.
9-720	A	41:17	:38	Recommend beginning the paragraph with the essential attribution statement, namely as line 37 reads "It is not yet possible to attribute changes in the MOC properties to natural and anthropogenic causes" [Govt. of United States of America (Reviewer's comment ID #: 2023-615)]	Accepted.
9-721	A	41:20		the caveat from the paragraph above is applicable here as well. [David Rind (Reviewer's comment ID #: 214-105)]	Accepted. Caveat added and reference made to chapter 5.
9-722	A	41:20		The caveat from the paragraph above is applicable here as well. [Govt. of United States of America (Reviewer's comment ID #: 2023-616)]	See 9-721
9-723	A	41:20		Suggest adding the words "over 5 samples taken" after "30%" [Richard Wood (Reviewer's comment ID #: 294-15)]	Accepted.
9-724	A	41:30	41:30	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1320)]	Rejected. Style preference.
9-725	A	41:31	41:31	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1321)]	Rejected. See 9-724.
9-726	A	41:34	41:34	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1322)]	Rejected. See 9-724.
9-727	A	41:38	41:38	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1323)]	Rejected. See 9-724.
9-728	A	41:40	43:12	Para 9.5.2: This is a poorly and confusingly written new paragraph. The information is hidden between too many numbers and references. What is the conclusion of this paragraph? [Fons Baede (Reviewer's comment ID #: 9-16)]	Accepted. Text substantially revised to improve clarity. Sea-level rise budget now detailed in a table.
9-729	A	41:42	:57	Give the relevant attribution knowledge in the lead sentence, rather than the procedural statement of what models must do, etc. [Govt. of United States of America (Reviewer's comment ID #: 2023-617)]	Accepted, attribution has been referenced. Some of the procedural description will be put in an appendix to Chapter 10, where it can be explained more fully.
9-730	A	42:1	43:12	Too many values…a table would be helpful.	Taken into account See 9-728

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				[Ronald J Stouffer (Reviewer's comment ID #: 258-27)]	
9-731	A	42:1		really only one large volcano in the 1960s, but several large volcanoes over the last 50 years. [David Rind (Reviewer's comment ID #: 214-106)]	Accepted.
9-732	A	42:1		Really only one large volcano in the 1960s, but several large volcanoes over the last 50 years. [Govt. of United States of America (Reviewer's comment ID #: 2023-618)]	Accepted.
9-733	A	42:29		why are intensified El Ninos related to warming of the western Pacific? [David Rind (Reviewer's comment ID #: 214-108)]	Not clear what text this comment refers to, so unable to respond.
9-734	A	42:31		is the model's contribution here just to give the temperature change globally? [David Rind (Reviewer's comment ID #: 214-107)]	Taken into account by explaining more fully in an appendix to Chapter 10.
9-735	A	42:31		Is the model's contribution here just to give the temperature change globally? Could say temperature results from he AOGCMs imply... [Govt. of United States of America (Reviewer's comment ID #: 2023-619)]	See 9-735
9-736	A	42:32		+ between 1993 and 2003 is a mistake. [Govt. of United States of America (Reviewer's comment ID #: 2023-620)]	Accepted.
9-737	A	42:49		Who is the "we" in "we obtain…"? Remove the personal pronouns. [Govt. of United States of America (Reviewer's comment ID #: 2023-621)]	Accepted.
9-738	A	43:12	43:12	I hope all the uncertainties in the above sectuion are two standard errors [VINCENT GRAY (Reviewer's comment ID #: 88-1324)]	Noted. Yes, they are in the SOD, but will be changed to 5-95 ranges.
9-739	A	43:14		For a definition of the various modes, refer here explicitly to Box 3.4 in Ch 3 and to the Glossary. [Fons Baede (Reviewer's comment ID #: 9-17)]	Reference to Box 3.4 added. We think an explicit reference to the glossary here is unnecessary.
9-740	A	43:18	:20	"the extent to which … profound." Nor sure what this sentence means? Impact of mode themselves is profound but it is uncertain how external forcing will alter them? Rewrite the sentence. [Govt. of United States of America (Reviewer's comment ID #: 2023-622)]	Order of clauses in this sentence reversed to clarify meaning.
9-741	A	43:20	:23	Why is there a prior expectation that modes will change? Also, what will change? [Govt. of United States of America (Reviewer's comment ID #: 2023-623)]	'can be expected' changed to 'might be expected' in response to this comment. The details of what might change are provided in the subsequent sections.
9-742	A	43:25		9.5.3.1: Why not go a step further and say that analyses of those models with relatively realistic ENSO variability is beginning to suggest that modifications in the ENSO cycle will be subtle rather than dramatic. We should not assume that there is a dramatic change in store but that the models have somehow not converged on it yet. [Isaac Held (Reviewer's comment ID #: 105-42)]	Rejected. This is a Ch 10 issue, and we now cite chapter 10's summary on this topic.

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9-743	A	43:26	:39	Begin para with the sentence (line 37-39) “There is no detectable change in ….” [Govt. of United States of America (Reviewer’s comment ID #: 2023-625)]	Rejected. Style issue.
9-744	A	43:29	43:29	The phrase "partly corresponding to a mean warming of the western equatorial Pacific" is suggestive of a link between this and the enhanced variability observed since the 1970s. While some studies have attempted to explain the increase in variability around this time in terms of a shift from westward-propagaing SST modes to eastward-propagating thermocline modes, there is still no consensus about the change. Also, it is difficult to identify changes in the mean beacuse of the large interannual variability hence one may question the conclusion of the one cited paper. [Matthew Collins (Reviewer’s comment ID #: 44-13)]	Clarified. Changed “partly corresponding to” to “which may reflect in part a”. Also ‘(conventionally defined as a mean SST anomaly in the eastern equatorial Pacific)’ has been added to clarify that the ENSO index is a mean SST anomaly in the eastern Pacific, and that a mean warming in this region will therefore necessarily cause a trend towards a more positive ENSO index.
9-745	A	43:29		What is the relationship between more intense El Ninos and warming in the western tropical Pacific? Is this just coincidental or is there a casual relationship implied? [Govt. of United States of America (Reviewer’s comment ID #: 2023-626)]	See 9-744
9-746	A	43:29		Why are intensified El Ninos related to warming of the western Pacific? Please clarify - is there causality here, or just a coincidence in time? [Govt. of United States of America (Reviewer’s comment ID #: 2023-627)]	See 9-744.
9-747	A	43:30	43:30	By what measure is the 1998 El Nino the "strongest on record"? I assume SST anomalies. Is this also the case if one looks at SLP anomalies? [Ronald J Stouffer (Reviewer’s comment ID #: 258-28)]	Sentence deleted.
9-748	A	43:31	43:36	The cited studies refer to future changes in ENSO and are dealt with extensively in chapter 10. Perhaps they are better dealt with there. [Matthew Collins (Reviewer’s comment ID #: 44-14)]	We use the projections as a way to provide an expectation of how El Nino might be expected to have changed. Two references to Chapert 10 are now included in this section.
9-749	A	43:32	43:32	This section duplicates 10.3.5.3 to a large extend, but the literature cited here is a few years behind the literature discussed in Chapter 10 and based only on TAR model results. It should be updated to include the results based on the 4AR models. [Govt. of Netherlands (Reviewer’s comment ID #: 2016-47)]	See 9-748.
9-750	A	43:36	:37	How does using a model relate to deducing what the observations are showing? Please explain. [Govt. of United States of America (Reviewer’s comment ID #: 2023-624)]	See 9-748.
9-751	A	43:41	44:3	This discussion of PDO is not obviously needed in so far as it has no attribution statements as now written. Perhaps the point here should be that there are strong, outwardly natural, decadal fluctuations in the Npac, and that these influence an ability to	Much of this section has been cut and replaced with references to chapter 3 and chapter 8. Additional caveats on

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				detect climate change at this point in time. Consider if the result from one model represents a real attributable change; if not, either give this as a conclusion or consider having this whole section discussed in Chapter 3. [Govt. of United States of America (Reviewer's comment ID #: 2023-628)]	detection results added.
9-752	A	44:2	44:3	What is the assessment of the MIROC result? [Ronald J Stouffer (Reviewer's comment ID #: 258-29)]	Caveats on Shiogama results added.
9-753	A	44:2	:3	What does, if any, observational evidence suggests? Do the observations suggest a positive phase influence - or is the model result only for the future? [Govt. of United States of America (Reviewer's comment ID #: 2023-629)]	Wording changed to clarify that Shiogama et al. deal with the observations.
9-754	A	44:5		9.5.3.2: Rather than talk so much about trends in NAM, etc, I would rather see this discussed more simply as a poleward shift in the circulation, but this seems to be a losing battle. [Isaac Held (Reviewer's comment ID #: 105-43)]	Rejected. The poleward shift of the stormtrack is referred to in this section. Most of the literature relevant to attribution of trends in the circulation deals with the NAM/NAO defined based on SLP. Therefore we retain these.
9-755	A	44:6		In chapter 8, pg. 30 line 1, there is a reference to the NAM as being 'not zonally symmetric', while here it says it is approximately zonally symmetric. The two chapters should be made consistent. [Govt. of United States of America (Reviewer's comment ID #: 2023-630)]	Rejected. For this reason we say 'approximately zonally symmetric'. The two chapters are not inconsistent.
9-756	A	44:19	:36	For clarity, replace "increase in the NAM index" with "increase in the positive polarity of the NAM index" [Govt. of United States of America (Reviewer's comment ID #: 2023-631)]	Rejected. The index itself has increased. It is not clear what 'increase in the positive polarity' means.
9-757	A	44:20	44:21	The citations here generally refer to simulations of future scenarios or in which the forcing is much stronger than experienced in the observed period, yet they are attached to a statement about observed changes. This should be qualified. [Matthew Collins (Reviewer's comment ID #: 44-15)]	Rejected. All the cited studies include simulations of 20th century climate change, and the latter four sources all focus on this period. The discrepancy between simulated and observed trends is discussed in the following clause.
9-758	A	44:46		high latitude SST changes, associated for example with sea ice reductions, also play an important role (hence both sea ice changes and NAO/AO changes feedback positively on one another-same references). [David Rind (Reviewer's comment ID #: 214-109)]	Accepted. 'tropical' deleted.
9-759	A	44:46		High latitude SST changes, associated for example with sea ice reductions, also play an important role (hence both sea ice changes and NAO/AO changes feedback positively on one another-same Rind references). [Govt. of United States of America (Reviewer's comment ID #: 2023-632)]	See 9-758.

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9-760	A	44:49	44:54	It might be useful to point out here that Knutson et al (listed as in press 2005 in reference list) show that in GFDL CM2.1 the underestimate in the Northern Asian 20th century warming is mostly alleviated if one adjusts for the fact that the model does not simulate much of a NAM trend. More generally, to the extent that models on average understimate the NAM trend, they should not be able to simulate the full observed polar amplification in the 20th century. Are models simulating too much polar amplification? [Isaac Held (Reviewer's comment ID #: 105-44)]	Rejected. This is a ch 8 issue.
9-761	A	44:49	:54	It is not clear that the increasing Eurasian temp of recent decades have been "caused" by the NAM change, via air mass tranport, or whether both have been driven by a third factor. Indications are that the NAM-sfcT relation over the past 100+ yrs has been nonstationary, and so the recent relation may be coincidental as far as cause-effect statements are concerned. See recent studies by Osborn (2004). The historical relationship provides feedback on the importance of NAO advection change, and should be considered. [Govt. of United States of America (Reviewer's comment ID #: 2023-633)]	Rejected. We do not attribute cause and effect, but only say that the two changes are 'associated'.
9-762	A	45:1	45:30	The evidence for the importance of annular modes for attribution studies may not be as strong as suggested, e.g. Fig. 3.6.7 appears to show a rather small trend in SAM over 1979-2005, with a negative trend in recent years. This section needs to be reassessed. [Govt. of Australia (Reviewer's comment ID #: 2001-366)]	A caveat regarding the change in SAM since 2000 has been added. However, we defer a detailed assessment of the observed trends to chapter 3.
9-763	A	45:14	45:30	Somewhere in this paragraph, you should note that the SH jet is poorly located in most models (See 8.3.2). [Ronald J Stouffer (Reviewer's comment ID #: 258-30)]	Rejected. Section 8.3.2 deals with the ocean component of models and is not relevant here. 8.4.1 argues that most models simulate the SAM well.
9-764	A	45:18	:22	No point in speculating on the mechanism for how the SP stratospheric AAO signal is communicated to the troposphere, as there are many competing theories. As now written, this Chapter appears to endorse the notion of altered propagation of planetary waves, by using the word "probably". There is no evidence that Im aware of that indicates this to be any more probable than other mechansism, including simple dynamical adjustment consistent with PV inversion. Should mention several mechanisms and use the word 'possibly'. [Govt. of United States of America (Reviewer's comment ID #: 2023-634)]	Discussion of mechanism deleted in response to this comment.
9-765	A	45:25		"Stone and Ffye, 2005" to "Stone and Fyfe, 2005" [Daithi Stone (Reviewer's comment ID #: 256-12)]	Agreed. Thanks.
9-766	A	45:27	:28	But there is no statistically significant cooling in annual average surface air temperatures anywhere in the Antarctic - see comment 7. [Steve Harangozo (Reviewer's comment ID #: 98-12)]	Text refers to summer and this is now clarified. Whether or not trend is statistically significant is moot, because we describe an observed trend.

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9-767	A	45:28	:30	It needs to be clarified that the peninsula warming attributable to the SAM is mainly thought to be on its eastern (Weddell Sea) side, i.e. not generally throughout the northern peninsula. See Marshall GJ, Orr A, van Lipzig NPM, King JC. 2006. The impact of a changing Southern Hemisphere Annular Mode on Antarctic Peninsula summer temperatures. Journal of Climate. in press. See also comment 10 for more background info. [Steve Harangozo (Reviewer's comment ID #: 98-11)]	Accepted. Reference added and text ammended to refer to eastern side of Peninsula.
9-768	A	45:28		There is a reference here to "Antarctic plateau cooling". See comments #64, #73 and #131 on this topic. [Adrian Simmons (Reviewer's comment ID #: 242-133)]	Accepted. We now say 'surface' warming over the Antarctic plateau to distinguish from the trend in the free troposphere.
9-769	A	45:42		what does this suggest? [David Rind (Reviewer's comment ID #: 214-110)]	The interpretation of this discrepancy is unclear. There is very little research to asses on this topic.
9-770	A	45:42		What does this suggest? [Govt. of United States of America (Reviewer's comment ID #: 2023-635)]	See 9-769.
9-771	A	45:47	45:47	Replace "climate change" with "changeof climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1325)]	Rejected. Style preference.
9-772	A	45:47		This para is started with the essential attribution statement; suggest a similar style, where possible, be used throughout Chapter 9. [Govt. of United States of America (Reviewer's comment ID #: 2023-636)]	Have tried to do this where appropriate.
9-773	A	46:1		what gives rise to this expectation, given that rainfall intensities are expected to increase, so diabatic heating would as well? [David Rind (Reviewer's comment ID #: 214-111)]	The expectation is based on modelling studies. 'expectation' changed to 'simulations'. Basis for expectation is also discussed earlier in the paragraph. Studies of the future are used to aid in the interpretation of changes observed to date.
9-774	A	46:1		What gives rise to this expectation, given that rainfall intensities are expected to increase, so diabatic heating would as well? This is an important point, and if truly believed, the expectation should be explained further. On a broader point, if this is an attribution chapter, why is the future being discussed anyway? [Govt. of United States of America (Reviewer's comment ID #: 2023-637)]	See 9-773.
9-775	A	46:4		Section 9.5.3.6. Good section on a controversial topic. [Kevin Walsh (Reviewer's comment ID #: 280-2)]	Noted. Thanks.
9-776	A	46:5	:39	This section on tropical cyclones needs a rewrite. This is arguably one of the more	Rewrite as per 9-781. We cross

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				important attribution challenges, made so by recent events especially. A clear lead statement of what is new knowledge is needed. I could be the that the section begins with what now line 24. However, my own sense is that this section fails to highlight the weight of evidence that the most intense cyclones have increased in recent decades, that such an increase has been modeled with a CGCM using an embedded hurricane model (Knutson et al.), and that theory predicts such an outcome based on thermodynamic considerations. While one cant entirely disagree with the last sentence (lines 37-39), this is an objectionable sentence nonetheless in light of what new knowledge has accrued since TAR, and it understates the science's current attribution capabilities. [Govt. of United States of America (Reviewer's comment ID #: 2023-638)]	reference Ch 10 on models. Reviewer's last sentence does not reflect concensus in TC research community.
9-777	A	46:19	46:19	"an increase in radiative cooling" should be " a decrease in radiative cooling" (See Section 9.5.4.2) [Masato Sugi (Reviewer's comment ID #: 259-2)]	Agreed.
9-778	A	46:24	46:39	There is a redundancy here with Ch. 3, and a somewhat different slant on the discussion. The SST-tropical storm regressions should be discussed either here or in ch. 3, but not both to avoid generating mixed messages. [Isaac Held (Reviewer's comment ID #: 105-45)]	Paragraph has been revised and now cross-references Ch 3 several times.
9-779	A	46:24	46:39	Some mention of the inconsistencies in the observational data should be made here. [Ruth McDonald (Reviewer's comment ID #: 173-30)]	The revised text uses words like "apparent" when talking about changes in cyclone frequency and duration, and notes difficulties in monitoring. .
9-780	A	46:24	46:39	Chan (2006) found that a "trend" in the north west Pacific was due to an interdecadal variation. [Ruth McDonald (Reviewer's comment ID #: 173-31)]	Accepted. Paper cited.
9-781	A	46:24	46:39	The comments by Landsea (Nature 438 2005) on Emanuel (2005) and the reply by Emanuel (2005, Nature 438) sjould be discussed here. [Ruth McDonald (Reviewer's comment ID #: 173-32)]	Agreed. Several other additional recent papers cited to improve balance.
9-782	A	46:28		in Chapter 3 the Emanuel result comes with caveats that the data set is short and there is evidence of higher values earlier in the period. [David Rind (Reviewer's comment ID #: 214-112)]	See 9-781. Point emerges from this discussion.
9-783	A	46:29		In Chapter 3 the Emanuel result comes with caveats that the data set is short and there is evidence of higher values earlier in the period. [Govt. of United States of America (Reviewer's comment ID #: 2023-639)]	See 9-781.
9-784	A	46:32	46:33	The phenomenon of the "Atlantic Multidecadal Oscillation" has been widely taken out of context and mis-applied to phenomena for which any explanatory role is dubious. Defined as in Mann and Park (1994) and Schlesinger and Ramankutty(1994) which used spatiotemporal signal separate techniques or models, respectively, to separate a possible	Text revised. Mann and Emanuel, Trenberth cited. See 9-778

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				oscillatory signal from trend, the phenomenon is observed to have little amplitude over the tropical North Atlantic (and therefore is unlikely to have any role in tropical cyclone frequency or intensity). However, a false apparent 'oscillation' is easily 'detected' in studies which define the AMO simply as the residual after linear detrending, as is the case for studies (e.g. Goldenberg et al, 2001) attributing tropical North Atlantic SST changes to the AMO. It has been shown (Mann and Emanuel, Eos, in press) that in such cases, the apparent "AMO" signal is likely an artifact of the linear detrending, since the forced changes in SST are not linear in time. There is a very strong sulphate aerosol cooling impact over the main development region (6-18N, 20-60W) during the crucial Aug-Oct season, estimated as -1.1 degrees C in one recent study [Hansen, J. et al (2005), Efficacy of climate forcings, J. Geophys. Res.,110, D18104,doi:10.1029/2005JD005776.] The competition between long-term GHG forcing, and this regionally and seasonally very strong negative forcing late in the 20th century, leads to a false apparent 'oscillation'. Other submitted work by Trenberth and by Santer et al comes to a very similar conclusion (i.e., that there is no evidence for an "AMO" influence on tropical Atlantic SSTs or tropical cyclone activity. [Michael Mann (Reviewer's comment ID #: 156-78)]	
9-785	A	46:41	:57	The model and observation discussion of the same phenomena (storm intensity and frequency) should be placed together in the same paragraph. In addition, the attribution statement at the end should contain the 'likely', 'not likely' terminology for IPCC. [Govt. of United States of America (Reviewer's comment ID #: 2023-640)]	Paragraphs merged. We don't consider that the statement that simulated and observed changes 'are broadly consistent' requires a quantification of likelihood, since it is not an attribution statement.
9-786	A	46:42	46:46	Add in cross reference to Chapter 10, Section 10.3.6.4. [Ruth McDonald (Reviewer's comment ID #: 173-33)]	Accepted.
9-787	A	46:44	46:46	The consistency between Lambert and Fyfe on one hand and Geng and Sugi on the other is not as large as implied: in the former intensity of winter storms is seen to increase, whereas the latter shows increased summer storm intensity. These changes originate from different assumed mechanisms! [Bart Van den Hurk (Reviewer's comment ID #: 274-129)]	Reference to Geng and Sugi removed, and reference to chapter 10 inserted.
9-788	A	46:48	46:56	All models I am aware of indicate that the poleward shift in storm track (particularly the eddy momentum fluxes assoicated with the storm track) is the essence of NAM/SAM variability and trends. If you are saying that the SAM trend has been attributed to ozone with some admixture of greenhouse gases, then you can say with equal likelihood that these forcings have shifted the storms polewards. [Isaac Held (Reviewer's comment ID #: 105-46)]	We now mention that models also simulate a poleward shift in the storm tracks, and we already note that the strom track shifts are associated with the annular mode trends. However, there are no studies which explicitly detect or attribute changes in storm

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					tracks to anthropogenic forcing, therefore we cannot do this in our assessment of the literature.
9-789	A	46:51		the SH storm tracks are associated with ENSOs and their movement (poleward or equatorward) varies in the different ocean basins with ENSOs. To the extent that there has been a trend toward more positive ENSOs, this will affect the storm track trends [David Rind (Reviewer's comment ID #: 214-113)]	Noted. Our focus here is on the overall poleward shift in the storm tracks, and its possible association with anthropogenic forcing. We are not aware with any studies which attribute this change to ENSO.
9-790	A	46:51		The SH storm tracks are associated with ENSOs and their movement (poleward or equatorward) varies in the different ocean basins with ENSOs. To the extent that there has been a trend toward more positive ENSOs, this will affect the storm track. [Govt. of United States of America (Reviewer's comment ID #: 2023-641)]	See 9-789
9-791	A	47:4	47:4	Replace "As" by "If" [VINCENT GRAY (Reviewer's comment ID #: 88-1326)]	Accepted. Text revised.
9-792	A	47:4		Suggest not beginning with a "predictive" statement….."As climate warms, the amount of moisture in the atmosphere is expected to rise…". This is an attribution chapter, after all. [Govt. of United States of America (Reviewer's comment ID #: 2023-642)]	Accepted. Now expressed as a hypothesis. See 9-791.
9-793	A	47:5	47:5	Unsert after "pressure" "at equilibrium" [VINCENT GRAY (Reviewer's comment ID #: 88-1327)]	Rejected. Reviewer provides no justification for this change.
9-794	A	47:5	47:5	Add at end "But the atmosphere is never in equilibrium" [VINCENT GRAY (Reviewer's comment ID #: 88-1328)]	Rejected. Reviewer provides no justification for this change.
9-795	A	47:8	47:8	Double the confidence figures if they are only one standard error [VINCENT GRAY (Reviewer's comment ID #: 88-1329)]	These are 95% confidence limits.
9-796	A	47:9	47:13	Should a note be add to say that the upper tropospheric water changes may be important in determining the climate sensitivity and point to the coming discussion? [Ronald J Stouffer (Reviewer's comment ID #: 258-31)]	Rejected. The focus here is on water vapour and its links to precipitation, and we do not want to lengthen this section.
9-797	A	47:18		Remove the predicitive statement: "….can be expected to". Suggest stating what has happened, and then addressing the attribution.. [Govt. of United States of America (Reviewer's comment ID #: 2023-643)]	Rejected. Previous review comments have suggested describing what we expect physically first.
9-798	A	47:24		"shows almost no trend" is misleading. If there is any trend, it is a decreasing trend. [Fons Baede (Reviewer's comment ID #: 9-18)]	Accepted. 'almost no trend' replaced with 'a non-significant decrease'.
9-799	A	47:28	47:28	Insert "warming associated with" between "While" and "greenhouse", and replace "over most of the globe" with "in global mean precipitation". [Govt. of Australia (Reviewer's comment ID #: 2001-367)]	Partially accepted. The cited studies simulate the response to greehouse gases, not just the warming associated

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					with them, therefore we do not accept the first suggestion. We accept the second, and have inserted 'in global mean precipitation'.
9-800	A	47:28	:33	Suggest also looking at Kumar et al. 2004, Journal of Climate, 17, 653. Also don't start with predictive statement, but observed statement. [Govt. of United States of America (Reviewer's comment ID #: 2023-644)]	We now also cite Kumar et al. See 9-797 for second comment.
9-801	A	47:35	47:36	The statement that "precipitation changes should be controlled by the energy budget of the troposphere" is misleading. This is true of the global mean, but not of regional or zonal means, which are, to first approximation, controlled by the increased horizontal moisture fluxes. There is too much emphasis here on the small global mean precip response. As is said elsewhere, this is not as interesting a quantity as the global mean temperature response, since the expected precip responses are not everywhere of the same sign. Furthermore, the increase in poleward moisture flux and its consequences are more robust across models than the small global mean increase. Averaging precipitation responses over too large an area reduces signal/noise. [Isaac Held (Reviewer's comment ID #: 105-47)]	Accepted. 'Global mean' inserted before 'precipitation'. Two paragraphs on global precipitation shortened to give less emphasis to global mean. We now also cite Held and Soden on the regional precipitation response.
9-802	A	47:35		taken literally, this discussion would imply that if short wave radiation isn't affected, there will be no change in global average precpitation regardless of what happens to greenhouse gases or atmospheric temperatures - is that what you really want to say? [David Rind (Reviewer's comment ID #: 214-114)]	We say that the increase in precip due to warming is *partly offset* by the decrease in radiative cooling. Thus precip is influenced by longwave forcing, but less than for a shortwave forcing. Also see 9-801.
9-803	A	47:35		Taken literally, this discussion would imply that if short wave radiation isn't affected, there will be no change in global average precipitation regardless of what happens to greenhouse gases or atmospheric temperatures - is that what you really want to say? [Govt. of United States of America (Reviewer's comment ID #: 2023-645)]	See 9-802.
9-804	A	47:42	47:43	"This mechanism therefore suggests that precipitation should respond more to changes in shortwave forcing than longwave forcing," is misleading. As I mentioned in the comment for FOD, the radiative cooling in the atmosphere generally increase when greenhouse gas (for example water vapor) is increased. As an exceptional case, the cooling decreases when CO2 is increased, because of the "overlap effect" of CO2 and water vapor absorption bands, as pointed out by Sugi and Yoshimura (2004). Following changes to lines 42-43 are suggested: "Sugi and Yoshimura (2004) further indicated that the reduction of cooling efficiency due to CO2 increase is a result of the "overlap effect" of CO2 and water vapor absorption bands. This mechanism therefore suggests that precipitation should respond more to	Accepted. 'longwave' replaced with 'CO2'. We already cite Sugi and Yoshimura (2004), but do not want to go into as much detail as the reviewer suggests.

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				changes in shortwave forcing (eg. volcanic eruption) than longwave forcing (CO2 increase)," [Masato Sugi (Reviewer's comment ID #: 259-3)]	
9-805	A	48:7	48:8	I think this statement is a little misleading, since the effects of volcanic forcing are transient whereas the changes in land precipitation due to greenhouse gas forcing are sustained; ie, there is probably no residual effects of Pinatubo on current land precip, but there is certainly some effect due to greenhouse gas increases. [Brian Soden (Reviewer's comment ID #: 245-12)]	Accepted. The revised text now indicates more clearly the time scale of the volcanic response in precipitation. See first paragraph of 9.5.4.2.1.
9-806	A	48:14	48:14	Figure 9.5.4b is not from Lambert et al. (2005) as they only examined the global land mean. [Seita Emori (Reviewer's comment ID #: 62-14)]	Accepted. It based on the data in Lambert et al. (2005), but was produced for the chapter. For this reason we say 'Adapted from Lambert et al. (2005).'
9-807	A	48:16	48:19	Delete sentence as it is a repeat of the sentence starting line 28 on page 47. [Govt. of Australia (Reviewer's comment ID #: 2001-368)]	Accepted. We have deleted the sentence on pg 47, ln 28.
9-808	A	48:16	48:19	This is a verbatim repetition of page 9-47, lines 28-31. [Fons Baede (Reviewer's comment ID #: 9-19)]	See 9-807.
9-809	A	48:17	48:18	References that better substantiate this sentence: line 17, after "drying of the subtropics" add "(Neelin et al 2003)"; line 18 after "in the equatorial region" add "within strong convection zones (Chou and Neelin 2004; Chou et al 2006) [J. David Neelin (Reviewer's comment ID #: 187-19)]	We have added a reference to Neelin et al. (2006) which we prefer since it compares simualted and osberved changes. We have also added two other references on the same subject.
9-810	A	48:20	48:20	"at the equator" - "At" should be "near". The increase occurs in the ITCZ. [Ronald J Stouffer (Reviewer's comment ID #: 258-32)]	Accepted. Suggested change made.
9-811	A	48:24		after "(Hulme et al 1998)." add "Chou et al (2005) suggest that part of the effects of aerosols are mediated by their effects on tropospheric temperature and thus are spread by atmospheric dynamics over consirable distances from the direct radiative impacts. [J. David Neelin (Reviewer's comment ID #: 187-20)]	Rejected. We do not have space to go into this level of detail here.
9-812	A	48:28	47:33	Section 9.5.4 is an excellent discussion of precipitation changes due to global warming. However, while global mean precipitation does increase, I'm not sure that models predict precipitation to increase over "most of the globe". Rather, in a recent paper by Held and Soden (2006: Robust responses of the hydrological cycle to global warming, J. Clim, in press), we argue that the primary impact of global warming is to enhance the existing patterns of P-E; that is wet regions become wetter and dry regions become drier. This relatively simple prediction for how precipitation changes is shown to be a robust result of the AR4 models and reflects the rapid rate at which the atmosphere moisture increases compared to the slower rate of global precipitation increase. Among the consequences of	Accepted. We now say 'global mean precipitation' instead of precipitation 'over most of the globe'. We now also cite Held and Soden and report their principal finding regarding an enhancement of climatological P-E.

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				this change are not only an ehancment of the existing patterns of P-E, but also an increased variance in P-E (ie more frequent droughts and floods) and a reduction in the strength of the atmospheric circulation. [Brian Soden (Reviewer's comment ID #: 245-11)]	
9-813	A	48:32		Is "streamflow" the same as "runoff"? These terms should be clarified and added to the Glossary. [Fons Baede (Reviewer's comment ID #: 9-20)]	'Streamflow' refers to individual rivers, whereas 'runoff' is integrated over an area. However, we now use only 'runoff' in this paragraph to avoid confusion.
9-814	A	48:35	48:38	The Gedney et al. paper is problematic. The runoff data set they used is partially synthetic, and the ability of the synthesis to portray trends properly has not been demonstrated. From a physical standpoint, the conclusion is undermined by the fact that the analyses did not model the extremely strong BL feedbacks (increased stomatal resistance leads to drier, hotter air, which negates the effect of the resistance change) or plant-growth feedbacks (increased water use efficiency, if it were realized, would lead to increased vegetation growth in water-limited ecosystems). The overwhelming strength of the PBL feedback is evident in (1) Budyko and others' semi-empirical water-balance relations, which leave no room for an influence of stomatal resistance and (2) numerous theoretical and empirical analyses of the equilibrium evaporation concept, which shows that evapotranspiration becomes independent of stomatal control given sufficient fetch and/or time. [P.C.D. Milly (Reviewer's comment ID #: 179-21)]	Rejected. The reviewer's comments are not supported by the literature. Numerous Free-Air CO2 Enrichment experiments show that evapotranspiration is generally decreased under higher CO2 (Conley et al, 2001; Triggs et al, 2004; Hunsaker et al, 2000; Long et al, 2006; Kimball et al, 1999, Field et al, 1995). "Budyko and other's semi-empirical water-balance relations" are simplistic, and more recent work with fully-coupled land surface-atmosphere models (Sellers et al, 1996; Betts et al, 1997; Cox et al, 1999) suggests that boundary-layer feedbacks do not negate the effects of stomatal closure. Moreover, other work with models including vegetation dynamics (Cramer et al, 2001; Leipprand and Gerten, 2006) suggests that stomatal closure effects are also not negated by plant growth feedbacks.
9-815	A	48:55	48:55	Once again, I do not undertand this statement about "mean precipitation". [Isaac Held (Reviewer's comment ID #: 105-48)]	'global' inserted before 'mean'.
9-816	A	49:1	49:2	This has been more thoroughly tested in: Pall, Allen, Stone. 2006. Testing the Clausius-Clapeyron constraint on changes in extreme precipitation under CO2 warming. Climate Dynamics.	This paper is not citable here, since its status is 'submitted'.

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				[Daithi Stone (Reviewer's comment ID #: 256-13)]	
9-817	A	49:2	49:5	To more correctly cite Emori and Brown (2005), the last part of this sentense should be changed to "… secondary in explaining the greater percentage increase in extreme precipitation than in mean precipitation that is seen in models." [Seita Emori (Reviewer's comment ID #: 62-12)]	Accepted. Suggested change made.
9-818	A	49:16	49:16	"Trenberth et al. (Trenberth et al., 2005)" should be "Trenberth et al. (2005)"? [Seita Emori (Reviewer's comment ID #: 62-13)]	Accepted. Suggested change made.
9-819	A	49:16	49:16	Type-o. Too many Trenberths. [Ronald J Stouffer (Reviewer's comment ID #: 258-33)]	See 9-818.
9-820	A	49:48	49:54	Isn't the low signal to noise ratio the reall issue here? [Ronald J Stouffer (Reviewer's comment ID #: 258-34)]	This is now mentioned.
9-821	A	49:48	:55	Begin this section with the sentence "There have been suggestions of anthropogenic influences on precipitation on the regional scale, as discussed below." Delete the remainder of this para. [Govt. of United States of America (Reviewer's comment ID #: 2023-646)]	Rejected. We prefer to include more background to give context to the rest of the section.
9-822	A	50:7	50:7	"internal variability" of what? Presumably the coupled land-atmosphere system? I guess not the coupled ocean-atmosphere sytem as this is the same as the SST 'hypothesis' already noted. Anyway, please clarify. [Dave Rowell (Reviewer's comment ID #: 222-46)]	Accepted. Re-phrased to clarify that it is the SST changes which could be of natural or anthropogenic origin.
9-823	A	50:13	50:14	these models do simulate the trend with the right sign but usually with much smaller amplitude. This is usually "masked" by using normalized indexes as with the figure 9.5.5. This is ok as long as the amplitude underestimation is clearly mentioned in the text. [Govt. of France (Reviewer's comment ID #: 2010-66)]	Accepted. The underestimation of Sahel precip changes by the models is now mentioned.
9-824	A	50:14	50:15	The reference to Held et al (2005) should instead be to Lu, J. and T. L. Delworth, 2005: Oceanic forcing of the late 20th century Sahel drought. Geophysical Research Letters, 32, L22706, doi:10.1029/2005GL023316. [Isaac Held (Reviewer's comment ID #: 105-49)]	Accepted.
9-825	A	50:20	50:22	this sentence should be shifted to the end of the section and combined with lines 40-43 on the same page as it is in the middle of the discussion about the various ocean basins influence. [Govt. of France (Reviewer's comment ID #: 2010-67)]	Reference to Hoerling et al.'s greenhouse gas results removed, but reference to internal variability retained, since this paragraph addresses the three main hypotheses for the causes of the trend in turn.
9-826	A	50:24	50:24	Fig 9.5.5. The reference for this figure should be changed to -- From Held, et. al (2005), based on the results described in Lu, J. and T. L. Delworth, 2005: Oceanic forcing of the late 20th century Sahel drought. Geophysical Research Letters, 32, L22706,	Accepted.

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				doi:10.1029/2005GL023316. [Isaac Held (Reviewer's comment ID #: 105-60)]	
9-827	A	50:26	:28	Is this quoting results from a single model? Add references to both the SSTs and, if available, land use part. [Govt. of United States of America (Reviewer's comment ID #: 2023-647)]	Rejected. References were cited in previous paragraph.
9-828	A	50:37	50:43	I suggest deleting these sentences (from "Held …"), because (a) these sentences themselves conclude that they provide an unlikely cause (hence they add little to our understanding, and (b) the performance of AOGCMs is vastly inferior to that of AGCMs in simulating recent decadal variability in this region (cf. Cook and Vizy 2006 [cited in Ch.11] and Hoerling et al. 2005a), because of their poor SST climatology in some key regions, and so I believe the projections of AOGCMs for the Sahel cannot yet be trusted. [Dave Rowell (Reviewer's comment ID #: 222-47)]	Rejected. Cook and Vizy (2006) show that many of the MMD models do not realistically simulate the climate of west africa, and therefore we have moderated our discussion of the hoerling et al. results, deleting the last phrase "limiting confidence in the Held et al. (2005) results. Nonetheless they find that some models do have a realistic climatology in this region, including the GFDL model used by Held iet al. However, our primary aim is to attempt to attribute observed clilmate change directly to changes in external forcing (rather than just SSTs), therefore it is imperative that we discuss coupled model results, even if results are inconclusive.
9-829	A	50:40	50:43	I think these lines are based on a misreading of Hoerling et al. My understanding is that the 20C3M AR4/PCMDI simulations do show a drying trend over the Sahel, albeit quite a bit smaller than that observed, but not that much smaller than the ensemble mean in Held et al, which then changes over to a modest increase in rain in the 21st century scenarios, in the ensemble mean. Since the latter must be due to greenhouse gases, it is safe to assume that the 20th century response in the ensemble mean is due to aerosols and/or volcanoes (most likely the former). So the difference is not that the 20th century anthropogenic drying trend in Held et al is uncorroborated by the other models-- it is plausible that there is an aerosol signal in the models on average, but what is uncorroborated on average is the drying in response to greenhouse gases (or, as described in Held et al) to a uniform warming of SSTs. [Isaac Held (Reviewer's comment ID #: 105-50)]	Hoerling et al.'s figure 10 shows no significant drying trend over the 1950-1999 period. However, in response to the comment we have moderated the wording of the sentences referred to.
9-830	A	50:42	50:43	if this is the conclusion of the paragraph (rather than simply Hoerling's conclusion), then the emphasis given to the aerosol cause in the first part of the paragraph seems	Partially accepted. Text revised.

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				misleading. [David Rind (Reviewer's comment ID #: 214-115)]	
9-831	A	50:42	:43	If this is the conclusion of the paragraph (rather than simply Hoerling's conclusion), then the emphasis given to the aerosol cause in the first part of the paragraph seems misleading. What is the assessment here? [Govt. of United States of America (Reviewer's comment ID #: 2023-648)]	See 9-830.
9-832	A	51:7	51:8	No reference is given for the statement that the rainfall trend in SW Australia is larger in winter than in summer. [Isaac Held (Reviewer's comment ID #: 105-51)]	Reference is given in in first sentence of paragarpah.
9-833	A	51:31	51:31	"thinning of the ice" Annual average? [Ronald J Stouffer (Reviewer's comment ID #: 258-35)]	According to chapter 4, the sampling of ice thickness is limited, therefore no information on the seasonality of the trend is available. We prefer to report the general conclusion of chapter 4 only on this topic.
9-834	A	51:31	51:54	Vinnikov et al. 1999 reference needs added. Vinnikov, K. Y., A. Robock, R. J. Stouffer, J. E. Walsh, C. L. Parkinson, D. J. Cavalieri, J. F. B. Mitchell, D. Garrett, and V. F. Zakharov, 1999: Global warming and Northern Hemisphere sea ice extent. Science, 286(5446), 1934-1937. [Ronald J Stouffer (Reviewer's comment ID #: 258-36)]	Reference added.
9-835	A	52:11	:14	But there is no statistically significant cooling in annual average surface air temperatures anywhere in the Antarctic - see comment 7. [Steve Harangozo (Reviewer's comment ID #: 98-13)]	Noted. This sentence refers to December-May.
9-836	A	52:26	52:27	The same features are shown in simulations of future climate change. Refer to Sections 11.3.3.3.7 and 11.3.5.3.4. [Govt. of Finland (Reviewer's comment ID #: 2009-111)]	Rejected. These lines of evidence are based on physical understanding of the climate system, and there is no need to use model simulations as supporting evidence here.
9-837	A	52:40	52:46	contradictory comments on these two lines: if winter accumulation positively correlated with temperature in the NH then why is there a decline in glacial mass balance with winter warming? [David Rind (Reviewer's comment ID #: 214-116)]	This is already addressed in the text: the temperature affect on melting is larger than its affect on precipitation.
9-838	A	52:40	:46	Contradictory comments on these two lines: if winter accumulation positively correlated with temperature in the NH then why is there a decline in glacial mass balance with winter warming? Is it melting in winter (seems unlikely)? [Govt. of United States of America (Reviewer's comment ID #: 2023-649)]	see 9-837.

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9-839	A	52:49	52:49	Replace "internal climate variuability" with "varous climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1330)]	Rejected. Suggested change would misrepresent cited study.
9-840	A	52:50	52:50	Replace "likely to be" by "possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-1331)]	Rejected. Reviewer provides no justification for this change.
9-841	A	53:12	53:13	Insert additional reference Lawrence and Slater, 2005. which explores the impact of permafrost, and ensure a reference to 9.5.5.5 is included in the Technical Summary. [Govt. of Australia (Reviewer's comment ID #: 2001-369)]	Rejected. We refer to Chapter 4 where the topic is discussed more fully.
9-842	A	53:19	53:19	Delete "quantitative" [VINCENT GRAY (Reviewer's comment ID #: 88-1332)]	Rejected. TAR did show quantitative results.
9-843	A	53:20	53:20	Delete "atmospheric and" There is no evudence from the atmosphere [VINCENT GRAY (Reviewer's comment ID #: 88-1333)]	Rejected. This is incorrect.
9-844	A	53:20	53:20	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1334)]	Rejected. Style preference.
9-845	A	53:20	53:20	Replace "identified" withn "postulated" [VINCENT GRAY (Reviewer's comment ID #: 88-1335)]	Rejected. Suggested change would misrepresent the large body of work and the conclusions expressed in these very many studies.
9-846	A	53:22	53:22	Replace "strengthening" by "promoting" [VINCENT GRAY (Reviewer's comment ID #: 88-1336)]	Rejected. Tendentious.
9-847	A	53:22	53:22	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1337)]	Rejected. Style preference.
9-848	A	53:22	53:23	Delete from "further" on line 22 to "models' on line 23 [VINCENT GRAY (Reviewer's comment ID #: 88-1338)]	Rejected. Reviewer provides no justification for this change.
9-849	A	53:25	53:25	Replace "have noe been shown" by "are" [VINCENT GRAY (Reviewer's comment ID #: 88-1339)]	Rejected. The wording draws attention to fact that this is a new development since the TAR.
9-850	A	53:25	53:26	Replace "simulated natural variability" with "several natural climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1340)]	Rejected. Reviewer misunderstands the natural variability here – clearly meant to be internal.
9-851	A	53:26	52:26	Insert before "consistent.."possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-1341)]	Rejected. Reviewer provides no justification for this change.
9-852	A	53:26	53:26	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1342)]	Rejected. Style preference.
9-853	A	53:27	53:27	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1343)]	Rejected. Style preference.

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9-854	A	53:28	53:28	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1344)]	Rejected. See 9-853.
9-855	A	53:28	53:28	Insert after "rise" "recently" [VINCENT GRAY (Reviewer's comment ID #: 88-1345)]	Rejected. Sea level rise observed for over a century.
9-856	A	53:32	53:32	Replace "variability " with "climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1346)]	Rejected. This change would be confusing and misleading.
9-857	A	53:35	53:35	Replace "detectabke" with "possible" [VINCENT GRAY (Reviewer's comment ID #: 88-1347)]	Rejected. Suggested change would misrepresnet studies cited earlier.
9-858	A	53:36	53:36	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1348)]	Rejected. See 9-853.
9-859	A	53:36		In what sense does the SAM result lead to a detectable human influence on GLOBAL surface pressure? Is the SAM associated with significant surface-pressure changes in the northern hemisphere? [Adrian Simmons (Reviewer's comment ID #: 242-134)]	This follows because the SH change dominates the global analysis. However, we have now replaced 'leading to a' with 'and there has been a', to avoid this misunderstanding.
9-860	A	53:36		Following on from the preceding comment, there is a human influence on global surface pressure that is not discussed in this chapter. As the atmosphere warms, and its water-vapour content increases, then its global-mean surface pressure will rise. There is evidence that this is detectable in reanalyses (Trenberth and Smith, 2005; discussed also in Uppala et al., 2005). The global-mean surface pressure analysis and the separately analysed global-mean mass of water vapour both increase from a minimum in 1986, consistent with the signal of upper tropospheric moistening shown in figure 3.4.6. [Adrian Simmons (Reviewer's comment ID #: 242-135)]	This effect is now discussed in section 9.5.3.4 and Trenberth and Smith is referenced.
9-861	A	53:38	53:38	Replace "an anthropogenic" with " a human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1349)]	Rejected. See 9-853.
9-862	A	53:40	53:42	The statement implies that the observed sea surface temperatures were due to anthropogenic influence. Need to provide corresponding citations. [Govt. of United States of America (Reviewer's comment ID #: 2023-651)]	Accepted. Chain of inference now better supported.
9-863	A	53:40		What does this imply for future changes in precipitation? Are they really related only to short wave variations? [Govt. of United States of America (Reviewer's comment ID #: 2023-650)]	Projections are not the province of this chapter.
9-864	A	53:41	53:41	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1350)]	Rejected. See 9-853.
9-865	A	53:44		see comment earlier about what this implies for future changes in precipitation - are they really related only to short wave variations? [David Rind (Reviewer's comment ID #: 214-117)]	See 9-863.

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9-866	A	53:56	53:57	Replace "simulated internal variability" with "some simulated climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1351)]	Rejected. Would misrepresent cited studies.
9-867	A	54:0	61:	significant improvement in terms of balance [Hermann Held (Reviewer's comment ID #: 104-14)]	Noted, thanks
9-868	A	54:4		Section 9.6. Unfortunately, there are still many problems with your discussion of climate sensitivity. Of course, this is substantially due to problems in the underlying literature, but nevertheless it will be a shame if you cannot provide a better assessment of the state of play. There are some serious problems in the way that the so-called "observationally constrained estimates" of climate sensitivity have been presented and interpreted in the literature. It seems likely that those on the outside of this field will have been misled into assigning more credibility to this work than it deserves. It is clear that the standard ansatz for generating the so-called "observationally constrained estimates of climate sensitivity" is fundamentally flawed in a way as to automatically assign unrealistically high probability to extremely high sensitivities. There really is no support for such a belief to be rationally held. The use of a highly restricted subset of available data must result in exaggerated uncertainties, which in the current context implies an unrealistic probability assigned to high sensitivities. Obviously our recent GRL paper (Annan and Hargreaves, GRL 2006) addresses this point in a rather simplistic manner - the manuscript was written in little more than a weekend after our shock at reading the first draft of the AR4 - but the point still appears incontestable. The vague waffle about how the assumptions of different studies may not be compatible hardly justifies completely discarding huge reams of data. Justifying this would require the argument (which is nowhere made in the literature, let alone credibly so) that the evidence which was not considered in the particular study is quite literally worthless. The Forster and Gregory paper (which we were unaware of at the time of writing Annan and Hargreaves 2006) is in our view particularly valuable as it does not rely on any numerical modelling at all, and so strongly enhances the growing mountain of evidence in favour of a mid-range climate sensitivity. [James Annan (Reviewer's comment ID #: 6-2)]	The reviewer comments on the use of uniform priors when discussing observational constraints. We have accepted parts of this comment, by adding a reference to the uniform prior assigning higher probabilities to high sensitivities than e.g. experts would do, and by adding the Forster and Gregory result using a uniform prior for sensitivity to figure 9.6.1. Part of the comment has been taken into account by more strongly emphasizing multi-evidence results (Annan et al. and Hegerl et al.). Comment that the discussion of ECS is biased towards high sensitivities rejected. As discussed, dependence between data and remaining similar uncertainties in most estimates make it problematic to combine all available lines of evidence. Failure to provide a tight constraint on ECS from observational constraints is not logically equivalent to a high probability of high sensitivities.
9-869	A	54:4		To recap, the standard approach has been to take some data set, and a so-called "ignorant" uniform prior in sensitivity S, and combine them using Bayes' Theorem to generate a supposed pdf. However, it is hard to see how such a result could credibly represent the beliefs of an informed scientist.	Choice of prior: The prior used for comparing results in figure 9.6.1 is uniform between 0 and 10, and the implications of a uniform prior have

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				The two basic problems are in the choice of prior, and the limited use of data. On the first point, it is not at all reasonable to claim that a uniform prior actually represents "ignorance", and ch9 p55 l6-7, ch9 p 97 l48- are rather unfortunate in this respect. It is incontestable that a uniform prior for S of the form U[0C,20C] as recommended by Frame et al actually represents a prior assertion that S>6C at the 70% ("likely") level. Even the [0C,10C] of some other authors puts P(S>6C) as four times as likely, a priori, as P(2.5C<S<3.5C). The limited justification of various authors on this subject seems to be little more than wordplay that cannot deny this trivial mathematical point. Choosing the prior is not straightforward, but this does not excuse completely unreasonable ones! We note that Chris Forest has already presented results using a more credible "expert prior", in 2002 and 2006. Unfortunately they were not strongly highlighted, in favour of the more exciting results when uniform prior was used. We recommend that they are included in some manner in Fig 9.6.1 (or separately), perhaps with our results (Annan and Hargreaves GRL 2006) and/or those of Hegerl et al (Nature, 2006). Even though none of these are beyond criticism, they do appear to be the only results that approach a credibly defensible pdf for climate sensitivity, in our opinion. Actually, the results of Forster and Gregory (Jclim, 2006) - even though they used a rather unconventional prior - also seem quite reasonable, although perhaps this is more by luck than design. It must be noted that a prior that is non-uniform in S (such as one which is uniform in L=1/S) in no way "rules out high sensitivity a priori", to use the popular phrasing. If there was actually any evidence of high sensitivity, then the posterior would show it even with a prior that is uniform in L! In respect of the use of data, in addition to our recent GRL paper (Annan and Hargreaves, GRL 2006) it might be worth pointing out some clear misunderstandings that have commonly occurred in the literature. It is absolutely not necessary for any specific data set to itself provide any sort of strong "constraint" (by which people seem to mean a firm limit when combined with a uniform prior) in order for it to strengthen and enhance an existing estimate when combined via Bayes' Theorem. For instance, the discussion of whether any one of the different "constraints" considered in Chapter 29, "Avoiding Dangerous Climate Change" can be used to "rule out" high sensitivity completely misses the point that they all point fairly clearly towards a mid-range value, and thus jointly they "rule out" high sensitivity with much higher probability than any one could do in isolation. You also clearly repeat this error in Chapter 9 when you talk about LGM data "supporting, but not improving" the range from other sources. Further, in discussing Wigley et al's volcanic simulations, you seem to argue that the broader uncertainties associated with two earlier eruptions somehow contradicts or casts doubt on the analysis of Pinatubo. In fact, nothing could be further from the truth - these weaker results actually support and strengthen (if only slightly) the Pinatubo result. There are some possible	been discussed. Also, the tendency for all lines of evidence to point to a best estimate around 3 has been pointed out, thus this comment is accounted for (see response comment 868). Also, problems with expert prior pointed out in Appendix B. Use of data: See response to 868. Text has been revised to more clearly indicate interpretation of results based on different lines of evidence. Sentence on implications of LGM-based estimates has been revised along lines of comment. Also, taken into account by pointing out several lines of evidence yielding most likely values around 3. This has now been more emphasized and writing on combining lines of evidence has been improved along lines of comment.

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				criticisms of this work, but the fact that different eruptions give different uncertainties around their (highly compatible) best guesses is not one of them! [James Annan (Reviewer's comment ID #: 6-3)]	
9-870	A	54:4		It should also be recognised somewhere in this section that the observationally-based analyses actually significantly strengthen our justified belief in a mid-range sensitivity in the region of 3C. The primary explanation for the "alarm" over the probability of high climate sensitivity is actually due to a change in the prior belief. Where scientists used to be comfortable in expressing a subjective belief that S was close to 3C, now they seem to have decided that the "objective" choice is to assert a strong prior belief in very high sensitivity (such as the uniform distribution), and then a limited use of data may fail to overturn this prior assumption. [James Annan (Reviewer's comment ID #: 6-4)]	Accepted by now discussing that progress has been made in attaching a likelyhood to previous expert beliefs about climate sensitivity from objective analyses. Rest noted.
9-871	A	54:4		section # 9.6 I think reads pretty well for the bits I understand. [David Sexton (Reviewer's comment ID #: 233-2)]	Noted, thanks.
9-872	A	54:7		The term "Equilibrium climate response" is wrong. The Glossary makes a distinction between ""equilibrium climate sensitivity" and "transient climate response". The use of these terms is loose in para 9.6 and should be improved. [Fons Baede (Reviewer's comment ID #: 9-21)]	Accepted. We have replaced both occurrences of loose language.
9-873	A	54:8	54:10	I object here, and I have objected in Ch 8, to taking a general term (transient climate response) and defining it to mean a specific example of a transient response. This is almost as bad as the UNFCCC defining "climate change" as "climate change due to human causes". [Danny Harvey (Reviewer's comment ID #: 101-60)]	Rejected. The definition of transient climate response is widespread in the climate research community, and has not been defined newly here. However, broader meaning of the definition has now been clarified to account for reviewer comment
9-874	A	54:15		refer specifically to Section 8.6 of Chapter 8 [Danny Harvey (Reviewer's comment ID #: 101-61)]	Accounted for by referring to section in line 13.
9-875	A	54:16	54:16	Typo: feebacks -> feedbacks [Hermann Held (Reviewer's comment ID #: 104-1)]	Accepted.
9-876	A	54:21	54:22	Replace "climate change" with "change of climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1352)]	Rejected. Simple style preference.
9-877	A	54:31	54:32	The paragraph above is one of the clearest explanations I have seen of the role of the climate state in influencing the climate sensitivity. The last sentence that this approach "circumvents the problem of feedbacks being dependent on the climate state" is perhaps a little over-ambitious though. Take for example the case of sea-ice feedback. It presumes that the relationships between the model parameters and the sea-ice feedbacks at the LGM	Suggested change accepted, thank you.

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				are the same as those relationships which operate now. I am not sure we can assume this apriori. Perhaps "circumvents" could be replaced by "ameliorates". [Matthew Collins (Reviewer's comment ID #: 44-16)]	
9-878	A	54:31	94:32	"at least to the extent that models realistically account for these changes" -- what basis is there for assuming that this condition is adeequately met to justify any confidence in the conclusions? [Paul Baer (Reviewer's comment ID #: 10-10)]	Confidence in this assumption is increased by results in sections 9.3-9.5, which is now referred to.
9-879	A	54:35	54:35	I think it should be noted that the estimates are based on "ensembles of simulations with climate models of varying degrees of complexity". There are still relatively few ensemble studies with GCMs. [Matthew Collins (Reviewer's comment ID #: 44-17)]	Accepted, change implemented.
9-880	A	54:36	54:41	While I think it would be nice to separate the discussion of constraints from climate changes from those obtained from mean climate state, I don't think this is currently the way the chapters are organised. Chapter 10 also discusses results from studies which use changes, citing many of the same papers cited in chapter 9. [Matthew Collins (Reviewer's comment ID #: 44-18)]	Accounted for by contacting chapter 10, who will refer to Sections in 9.6 instead.
9-881	A	55:6	55:7	This sentence is wrong. Please don't just reword it, but also consider how it has affected the validity of the work that relies on this incorrect assumption. [James Annan (Reviewer's comment ID #: 6-5)]	Sentence has been edited. For wider effects see response to comments 868-869.
9-882	A	55:14	55:16	We have a comment in review at GRL on Frame et al (GRL,2005) which points out (inter alia) that such "estimates" do not actually satisfy the Kolmogorov Axioms, in other words they cannot reasonably be described as "probability" at all. [James Annan (Reviewer's comment ID #: 6-6)]	Accounted for, the text does not call such priors probability distributions, but just "distributions".
9-883	A	55:14	55:16	The uniform prior as used in these studies is not what statisticians would call "uninformative", since it may give decisive information, e.g., on upper bounds on climate sensitivity. [Michael Mann (Reviewer's comment ID #: 156-80)]	Noted.
9-884	A	55:18		I am relieved that Frame et al. (a valuable contribution in itself) is now discussed balanced by trained statisticians such as Goldstein & Rougier. [Hermann Held (Reviewer's comment ID #: 104-5)]	Noted, thanks for the comment last round.
9-885	A	55:20	55:24	The uniform priors used in these studies can be rather restrictive, particularly in the estimation of upper bounds on the climate sensitivity. They may be overly restrictive, in that most of the information in upper bound estimates comes from the prior rather than from data. [Michael Mann (Reviewer's comment ID #: 156-79)]	The studies referred to in that section use multiple lines of evidence and are therefore only marginally influenced by the prior. The comment probably referrs to a few lines up, text has been inserted to clarify that the limits of the prior reflect

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					computer time limitations and are generally wide enough to encompass ranges experts consider plausible.
9-886	A	55:20		I believe the citation here should be to Annan and Hargreaves 2006 [Paul Baer (Reviewer's comment ID #: 10-11)]	Accepted, thanks.
9-887	A	55:21	55:23	I think it should be stressed that "multiple and independent lines of evidence" can only be combined if the constraints provided are truly independent (whatever that means). My worry is that some lines of evidence, e.g. changes in tropical temperatures at the LGM and historical trends, are essential constrains on the same physical feedbacks i.e. those associated with tropical clouds. I have discussed this issue with a number of colleagues and it is unclear to us what independent *really* means e.g. do the models, observational type, geographical extent etc. all have to be independent? Perhaps some of these issues could be expanded upon in consultation with Annan and Hargreaves. This is a potentially powerful but very new approach so the usual high levels of IPCC caution should be exercised. [Matthew Collins (Reviewer's comment ID #: 44-19)]	Accounted for. We agree that this is new and has to be treated with caution, which has been done in strongly limiting the impact of these results on our overall assessment. Also, the discussion of data independence has been expanded.
9-888	A	55:35	55:36	"the true uncertainty"? I hope I don't need to spell out why this is a poor choice of words. [James Annan (Reviewer's comment ID #: 6-7)]	Accepted, text revised.
9-889	A	55:41	57:51	Although you cite Frame et al. (2005), the most important point from their work is not discussed, namely, that the so-called "objective" pdfs are highly dependent on prior assumptions, which are themselves quite arbitrary. I think that too much attention is devoted to these pdfs, in light of these results, and I would greatly shorten this section. [Danny Harvey (Reviewer's comment ID #: 101-62)]	Taken into account one paragraph up, where the sensitivity of results to prior is discussed. We have shortened this section slightly, but not drastically, since estimates of climate sensitivity is a major advance since the TAR.
9-890	A	55:48		add "Harvey and Kaufmann (2002)", as we also compared the uptake of heat by the ocean component of our model with observations for different sensitivities. [Danny Harvey (Reviewer's comment ID #: 101-54)]	Accepted.
9-891	A	56:8	56:10	This should be rewritten as: "For example, Harvey and Kaufmann (2002) find a best-fit climate sensitivity of 2 C out of a plausible range of 1-5 C, and conclude that the fossil fuel and biomass aerosol radiative forcings in 1990 were unlikely to have exceeded -1 W m-2 and -0.5 W m-2, respectively" [Danny Harvey (Reviewer's comment ID #: 101-55)]	Partly accepted, partly accounted for (constraints on forcing are listed in 9.2.1.2 as crossreferenced)
9-892	A	56:10	56:14	I think that your representation of Lindzen and Giannitsis (2002) is a little too simplistic, and your dismissal of their work rather weak. Although I disagree with the conclusions in their paper, I think that their work should be discussed more fully, given that Lindzen is one of the most vocal critics of the IPCC and of the overwhelming scientific consensus	Accepted, text has been modified. Thank you.

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				represented in the IPCC reports. L & G observe that global mean tropospheric temperature reconstructed from radiosonde observations shows an abrupt upward shift in the mid 1970s, just before satellite (MSU) observations began. They suggested that the reason why the MSU data show less warming after 1979 than the surface observations is because the response of the surface to the atmospheric warming lagged behind the warming. They argue that the lag between the tropospheric and surface temperature responses can be used to calibrate longterm climatic sensitivity to external forcings because the time constant in the temperature response depends on climate sensitivity. You dismiss their work on the grounds that there is no reason for the surface temp to lag the atm temperature. But, in fact, the CO2 forcing is largely to directly warm the atmosphere (there being very little extra downward IR until after the atm has warmed). Because of the greater thermal inertia of the surface, it is not unreasonable that the surface warming could lag the atm warming. The abrupt change in atm temp could be due to some non-linearity in the system. My objections to the L &G paper are, instead, based on the following: (1) They use a very large ocean diffusion coefficient (1.5 cm2/s, I think) compared to what I would regard (based on my own modeling work) to be a much more realistic value (0.2 cm2/s in the upper thermocline) (a smaller diffusion coefficient permits a larger climate sensitivity), (2) there are too many uncertainties in the data to draw firm conclusions, and (3) the model used (1-D) and the experimental design (imposing a fixed change in atm temp with no ongoing external forcing) is too simple to draw firm conclusions. (Note: I don't have the paper in front of me, as I've misplaced it and don't have access to GRL online, so someone should check what diffusion coefficient value they actually used) [Danny Harvey (Reviewer's comment ID #: 101-56)]	
9-893	A	56:26		Typo: implement blank behind ")" [Hermann Held (Reviewer's comment ID #: 104-2)]	Accepted
9-894	A	56:57		Typo: implement blank behind ")" [Hermann Held (Reviewer's comment ID #: 104-3)]	Accepted.
9-895	A	56:57		is that what IPCC meant by the uncertainty range? [David Rind (Reviewer's comment ID #: 214-118)]	Accepted, text has been modified.
9-896	A	56:57		Is that what IPCC meant by the uncertainty range? [Govt. of United States of America (Reviewer's comment ID #: 2023-652)]	Accepted, text has been modified
9-897	A	57:10	57:12	It would be good to have some information on how the PDFs have been rescaled to have both uniform priors in each case and to integrate to unity over the 0-10C range. For the latter, I would expect the upper tail to be sensitive to this rescaling as in some cases the 95%-tile is close to that. There appears to be some considerable massaging of the PDFs in	Partly accepted, text has been modified. Note that this figure is similar to a published comparison in Hegerl et al., 2006 (now referenced in caption).

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				the figure so the information on how it was done should be recorded somewhere for completeness. [Matthew Collins (Reviewer's comment ID #: 44-20)]	
9-898	A	57:10	57:14	Explain the logic behind setting an upper limit of 10C. Explain (or point to a reference) why a negative sensitivity is unstable. [Gareth S. Jones (Reviewer's comment ID #: 121-94)]	Accepted, text has been modified and text on negative climate sensitivity has been clarified.
9-899	A	57:13	57:14	Negative climate sensitivity does not necessarily lead to an unstable climate, just a different climate from the one we know. Defined as the incremental response of e.g. global mean surface temperature to a change in global mean surface radiative forcing relative to some pre-existing equilibrium, a negative sensitivity would be obtained by reversing the sign of the true temperature change while preserving the sign of the radiative forcing perturbation. This would be a physically very odd climate (e.g. one which warms some small amount when the solar constant is decreased some small amount) but there are certainly no implications here for the stability properties of the system with respect to incremental perturbations. [Michael Mann (Reviewer's comment ID #: 156-81)]	Rejected, negative climate sensitivity in simple heat balance models of the climate system lead to uncontrolled growth of perturbations rather than decay. However, text has been clarified to circumnavigate this issue due to space limits.
9-900	A	57:13	57:14	"and is unconsistent with the climate" is vague. Change to "is an unphysical result". [Ronald J Stouffer (Reviewer's comment ID #: 258-37)]	Accepted, text revised along line of suggestion
9-901	A	57:21	57:22	As discussed by Harvey and Kaufmann (2002), the results obtained by Andronova and Schlesinger are highly dependent on the way they have separated forced from internal variability, which is related to their choice of external forcings. I do not find theirs and similar results to be at all credible, and these results should be treated with much more scepticism (esp in light of Frame's results) [Danny Harvey (Reviewer's comment ID #: 101-63)]	Accepted, discussion has been shortened.
9-902	A	57:23		Typo: implement blank behind "(" [Hermann Held (Reviewer's comment ID #: 104-4)]	Accepted
9-903	A	57:29	57:30	Maybe Knutti couldn't constrain ocean mixing parameters based on ocean heat uptake, but there are many other ways of constraining them- namely, the simultaneous simulation of the bomb and natural 14C distribution, the distribution of nutrient and O2 tracers, and the distribution of CFCs. These additional constraints narrow the allowable mixing parameters, which in turn narros the range of allowable climate sensitivities. An example (among many) of the use of multiple constraints in a quasi 1D model is Harvey, L.D.D. and Z. Huang (2001, "A quasi-one-dimensional coupled climate-carbon cycle model, Part1: Description and behavior of the climate component", Journal of Geophysical Research – Oceans 106, 22339-22353) and Harvey, L.D.D. (2001,"A quasi-one-dimensional coupled climate-carbon cycle model, Part II: The carbon cycle component", Journal of Geophysical Research – Oceans 106, 22355-22372, 2001). In these papers, a	First part accepted, writing has been revised and substantially shortened, since ocean diffusivity is not a main concern of this section. Second part rejected, this section deals with probabilistic estimates of climate sensitivity and therefore focuses on work determining pdfs of climate sensitivity, while related work has been referenced.

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				rather small diffusion coefficient and upwelling velocity is deduced, which in turn will rule out climate sensitivities much greater than 4 K. Overall, then, this section is too narrowly focused on the work of the handful of people who have been generating pdfs of climate sensitivity, to the exclusion of other work. [Danny Harvey (Reviewer's comment ID #: 101-64)]	
9-904	A	57:29	57:32	This description of the Knutti et al.and Forest et al. results is inaccurate. First of all Knutti et al. did not attempt to constrain the heat uptake by comparison with observations. They only constrained the climate sensitivity and the forcing. Second, Forest et al. (2002) did try to constrain it, but did not succeed in finding any meaningful constraint. Third, Forest et al. (2006) did find a constraint, but it is not stated accurately here. What they found was that the mixing into the DEEP ocean, below the mixed layer, was too efficient. The caveat in the current text that "this finding is uncertain due to structural model uncertainty" is rather superficial, given the very close agreement between the performance of the 2D ocean model used by Forest et al. and the performance of OGCMs in coupled AOGCMs shown by Forest et al. (2006) and by several other papers cited by them. In addition the Forest et al. conclusion is supported by the results shown in Fig. 9.5.1, which compares the observed global ocean warming with simulations by two of the same AOGCMs analyzed by Forest et al. (the two figures on the extreme right of fig. 9.5.1). The ensemble mean warming below the mixed layer is about 2 times the observed warming in the PCM, and about 2 1/2 times the observed warming in the HadCM3. A more relevant limitation on the Forest et al result is that their comparison of the models with observations does not contain most of the AOGCMs used for the AR4 projections. I suggest that the current sentence (lines 29-32) be replaced by the following three sentences: "Forest et al. (2006) find that many coupled AOGCMs are overestimating how rapidly heat is being mixed into the deep ocean (below the mixed layer). The overestimate of the warming of the global ocean by two of the models included in the Forest et al analysis is shown explicitly in Fig. 9.5.1. However most of the AOGCMs being used in the AR4 projections were not included in the Forest et al. analysis." [Peter Stone (Reviewer's comment ID #: 257-6)]	Accepted, writing revised similar to suggestion. Discussion of ocean mixing in Knutti et al has been removed.
9-905	A	57:46	57:51	I would suggest to remove the last line ("Forster and Gregory (2006) tentatively find that…as in models). [Sandrine Bony (Reviewer's comment ID #: 25-2)]	Accepted.
9-906	A	57:49	57:51	Perhaps you should emphasize that this is negative net (water vapour + clouds) feedback, not necessarily a negative water vapour feedback. [Govt. of Finland (Reviewer's comment ID #: 2009-112)]	Sentence has been removed.

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9-907	A	57:49	57:51	The statement that the sign of long wave feedback might be wrong in models needs some further explanation and quantification, given that the report elsewhere emphasizes the robustness of water vapor feedback and the fact that low clouds, with their relatively small infrared effect, dominate the intermodel differences in cloud feedbacks. The reader needs more guidance as to how seriously to take this result. [Isaac Held (Reviewer's comment ID #: 105-52)]	The senstence has been removed.
9-908	A	58:1	58:4	Should describe what the model was. The model used in Soden 2002 was an atmospheric GCM coupled to a mixed-layer ocean [Gareth S. Jones (Reviewer's comment ID #: 121-95)]	Accepted.
9-909	A	58:18	58:19	"suggest different upper limits". You seem to be falling into the trap of objectifying "the pdf of climate sensitivity" as something to be estimated here. But regardless of what you were thinking, the other eruptions support and strengthen the Pinatubo result, if only slightly - they certainly in no way contradict or cast doubt on it. [James Annan (Reviewer's comment ID #: 6-8)]	Accepted, the reference to different upper limits has been removed. However, the reference to missing uncertainties, as pointed out by Wigley, is important and has been kept.
9-910	A	58:27	58:28	Section 8.6 finds that volcanic eruptions provide some test of climate feedbacks, however it is limited due to differences in the level and distribution of forcing, uncertainties in observations, differences in land/sea response, etc. Therefore, replace provide a "powerful test for feedbacks" with provide a "useful test for some feedbacks" and reference Section 8.6. [Govt. of Australia (Reviewer's comment ID #: 2001-370)]	Accepted, thanks.
9-911	A	58:35	58:37	Use verbatim the definition for "transient climate response" in the Glossary. [Fons Baede (Reviewer's comment ID #: 9-22)]	Rejected. Other comments requested removal of this relevant section because reviewers did not appreciate the usefulness of the concept of transient climate responese. Therefore the text now references explicitly the glossary for definition, and explains what TCR is useful for.
9-912	A	58:46		typo: . . -> . [Hermann Held (Reviewer's comment ID #: 104-6)]	Fixed
9-913	A	59:2	59:2	The value refers to the warming at the doubling of CO2 in 70 yr, not in a century [Govt. of Finland (Reviewer's comment ID #: 2009-113)]	Accepted, text has changed.
9-914	A	59:36	59:38	Whilst Hegerl 2006 does include a measure of the uncertainty in solar and volcanic forcing, that uncertainty is itself uncertain. They assume a range of possible solar/volcanic forcing amplitudes, which has some evidence to support it but is rather weakly constrained. For instance looking at a range of volcanic reconstructions gives a wider range than assumed by Hegerl 2006. Recommend that this is re-phrased to reflect this	Accepted, text has been edited. Note, however, that uncertainties in volcanic reconstructions will be overestimated if based on Robertson's estimate using far fewer ice cores than newer estijmates,

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				uncertainty on uncertainty, e.g. "...incorporate one measure of uncertainty ..." [Gareth S. Jones (Reviewer's comment ID #: 121-96)]	so we disagree with the reviewer.
9-915	A	59:36	59:38	The Hegerl 2006 study does not implicitly include the range of possible efficacies in the solar and volcanic responses (see figure 2.23) that has been found in other modelling studies, this would impact on the resultant PDF. Perhaps this should be noted here. [Gareth S. Jones (Reviewer's comment ID #: 121-97)]	As discussed in supplement of Hegerl et al., uncertainties in efficacy have been folded into the estimate of overall uncertainty. This has been clarified, uncertainties have been stronger emphasized.
9-916	A	59:38	59:38	Replace "shape" with "time evolution" [Govt. of Finland (Reviewer's comment ID #: 2009-114)]	Accepted.
9-917	A	59:38	59:40	It is stated that only one reconstruction accounts for loss of variance due to calibration. I believe that this is a reference to a claim made in Hegerl et al (2006) that the method of "total least squares" ("TLS"), unlike other methods, can somehow provide statistical reconstructions of past surface temperatures from proxy data that preserve the true variance in the reconstructed series on some desired "low-frequency" timescale. This claim is fundamentally problematic for at least three reasons: [1]. Total least squares leads to less biased estimates of regression coefficients (they are unbiased under assumptions that are not satisfied in the Hegerl et al. study, such as known error variances), but reconstructions that fill in missing temperature values with (conditional) expected values always have lower variance than the actual temperatures because the missing temperature values are imputed from the center of the posterior distribution. That is, the sample variance of the reconstruction is *always* smaller than the actual variance by at least the variance of the reconstruction residual (see Little and Rubin, Statistical Analysis with Missing Data, Wiley, 2002); [2]. Rutherford et al. [Rutherford, S., Mann, M.E., Osborn, T.J., Bradley, R.S., Briffa, K.R., Hughes, M.K., Jones, P.D., Proxy-based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Methodology, Predictor Network, Target Season and Target Domain, Journal of Climate, 18, 2308-2329, 2005] use a regression approach (Regularized Expectation Maximization--Schneider, T. Analysis of incomplete climate data: Estimation of mean values and covariance matrices and imputation of missing values, J. Climate, 14, 853-871, 2001), that is a regularized total least squares regression and have shown that the method performs very well in practice for quite low signal-to-noise ratios [Mann, M.E., Rutherford, S., Wahl, E., Ammann, C., Testing the Fidelity of Methods Used in Proxy-based Reconstructions of Past Climate, Journal of Climate, 18, 4097-4107, 2005], so others have used similar (and quite arguably superior) approaches to that of Hegerl et al. (2006); [3]. Hegerl et al. select proxies on the basis of their correlation with instrumental temperatures, without cross-validation, leading to selection bias, an overestimation of correlations between proxies	Rejected. The comment is not relevant for the discussion commented on. However, we have added an "attempt to" since we recognize that uncertainty ranges for the amplitude of reconstructions are uncertain, and reference now a paper describing the ranges.

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				and temperatures, and an underestimation of imputation errors and hence of temperature variances (which must include a contribution from imputation error variances if expected values are filled in for missing values). [Michael Mann (Reviewer's comment ID #: 156-11)]	
9-918	A	59:43	59:43	What is the "new reconstruction" is this a new reconstruction created by Esper or is it one by another author. Should be referenced somewhere. [Gareth S. Jones (Reviewer's comment ID #: 121-98)]	Reference to new reconstruction replaced by citation.
9-919	A	60:12	60:53	Despite the uncertainties in these studies, it is important to note that they are at least complementary, and perhaps preferable, to studies attempting to constrain the PDF of climate sensitivity from temperature changes of the past millennium. For the LGM, we are looking at a quasi-equilibrium response of the climate to substantially altered boundary conditions. Compare this to the past 1000 years, where the sensitivity estimates, in most cases, are primarily a product of the short-term transient response to one particular (and uncertain) forcing (explosive volcanism) under b.c.s that are not all that different from today. I'm personally more comfortable in believing constraints from the prior case than the latter case. I think the discussion here loses focus of this key point. [Michael Mann (Reviewer's comment ID #: 156-12)]	Accepted. We have added a reference to quasi-equilibrium and boundary conditions of LGM based estimates. However, we are surprised by the impression of the reviewer that we give more weight to the last millennium results. Both are comparable in length of section referring them, and uncertainties in both are equally discussed.
9-920	A	60:14		typo:).T ->). T [Hermann Held (Reviewer's comment ID #: 104-7)]	Fixed.
9-921	A	60:28	60:53	The disagreement between Annan et al. and Schneider von Deimling et al. is not only due use of different models but also due to more realistic boundary conditions in the study by Schneider von Deimling et al.: in their study, LGM-specific dust & vegetation forcing is accounted for what is not the case in Annan et al. (as it is not in the PMIP-2 protocol). Schneider von Deimling et al. also estimated the magnitude of this effect in their publication: proper accounting for both boundary conditions shifts inferred climate sensitivity down by a value up to 1.4°C. Hence, the values derived by Annan et al. would have to be lowered by up to 1.5°C. Keeping this in mind, the discrepancy between the two studies does not look that large any more (compare also Fig 9.6.1, p.9-119). Hence I strongly disagree that nothing can be learned from current LGM studies on climate sensitivity yet: Quite the contrary, they show that 3 independent sources (Annan et al., Schneider von Deimling et al.-tropics, Schneider von Deimling et al., Antarctica) are compatible with CS=1.5...4.5°C and strongly disagree with values above 5°C. [Hermann Held (Reviewer's comment ID #: 104-8)]	Accepted, reference to the differences in forcing has been added. We also keep the other sources of uncertainties to discuss further limitations.
9-922	A	60:28		A limitation of our study is that we used the standard PMIP2 protocol, which as discussed elsewhere in the chapter does not actually (by design) provide a very good approximation to LGM boundary conditions. In particular, the enhanced negative forcing of more realistic dust and vegetation changes could be expected to have cooled our simulated	Accepted, reference to more realistic forcing in Schneider von Deimling has been added to explain differences in pdfs. Last sentence of the paragraph has

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				tropical SST significantly (pers comm Thomas Schneider von Deimling, and it's obvious enough from first principles), which would have led to us rejecting our high sensitivity models more strongly and thereby closed the gap between our results and those of Schneider von Deimling et al. Of course, there is still some uncertainty in the results, but not in our opinion enough to justify your subsequent statement on line 51. [James Annan (Reviewer's comment ID #: 6-9)]	been revised.
9-923	A	60:43	60:43	The study of Schneider von Deimling et al. infers a 5-95% of CS of 1.2 to 4.3°C when accounting for parameter uncertainty as well as forcing uncertainty (the stated upper limit of 3.5° does not account for forcing uncertainty). When structural uncertainties are considered the upper limit has to be increased by an additional 1°C as stated in the paper. [Hermann Held (Reviewer's comment ID #: 104-17)]	Partly accepted. Text emphasizes importance of structural uncertainty, but does not reference the estimate of 1oC since this seems a rather ad hoc estimate, and the point is better made by juxtapositioning Annan and Schneider von Deimling results..
9-924	A	60:45		"The discrepancy between the inferred upper limits in the two studies arises from structural differences between the models used: the model used by Annan et al. (2005) shows a much weaker connection between simulated tropical SST changes and climate sensitivity than that used by Schneider von Deimling et al. (2006)." That is not correct: the discrepancy arises because (1) Annan et al. used an atmosphere-only model, which thus ignores changes in ocean heat transport which were shown to be important for glacial climate by Ganopolski et al. 1998; (2) none of the models in the Annan ensemble had a climate sensitivity below 4 degrees, so by definition this study cannot possibly find a sensitivity below 4, and (3) only a subset of glacial forcings was used, ignoring dust and vegetation. Both the neglected processes (ocean heat transport change, missing forcings) are first-order processes which would have lead to a much lower climate sensitivity estimate for the same data constraints. The role of IPCC is to make an assessment, not a comprehensive review of every single publication. This involves the need to select the references with state-of-the-art methodology and robust and relevant conclusions. Therefore I would not cite the Annan et al paper in the report, as no robust conclusions can be drawn from it. At the very least the above caveats should be stated clearly. [Stefan Rahmstorf (Reviewer's comment ID #: 206-30)]	Partly accepted, text has been revised and emphasis slightly changed between both results. However, we believe that both papers significantly contribute to our understanding of climate sensitivity and uncertainties in estimating it, therfore suggestion to remove Annan et al. rejected.
9-925	A	60:51	60:52	"broadly support but cannot further constrain" appears to broadly misunderstand the function of separate analyses in tightening the overall estimate. The LGM data cannot help but "further constrain" the overall estimate, unless it leads to a uniform likelihood (which no-one, no-where, has ever argued to be the case) or is in somews fully co-dependent with the more modern data that has been more widely used (ditto). The arguments which have appered in the literature arguing that it isno use because it also "cannot rule out high S" are fundamentally and trivially invalid.	Accepted, text has been revised.

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				A simple way to look at this is to consider what would have happened if the estimates using recent data had used an LGM-derived prior, rather than a uniform one. Even a rather broad prior (to take account of the substantial uncertainties) would hardly assert P(S>6)=40%, as U[0,10] does. Forest's results where he used an expert prior give a good indication of the plausible outcome, and deserve to be more strongly featured. [James Annan (Reviewer's comment ID #: 6-10)]	
9-926	A	60:54		One more, brief subsection can be added here: "9.6.3.3 Inferences from the modelling of atmospheric CO2 variations over phanerozoic time". Although the IPCC has assembled an impressive body of evidence here (and in Chapter 8) concerning the climate sensitivity, and although it is clear that there is no credible scientific argument for the sensitivity being less than 1 K and very little support for it being less than 2 K, there is even more evidence than what is discussed in AR4. Namely, Bob Berner's work modelling CO2 variations during the past 600 million years. The magnitude of past CO2 variations in response to changing rates of weathering due to variations in plate tectonic activity and associated mountain uplift depends on how strong the negative feedback is between CO2 concentration and removal rates by chemical weathering, and this in turn depends on how large the climate sensitivity is (warmer temperatures drive higher weathering rates). If climate sensitivity is too small (less than 2 K), impossibly high CO2 peaks are obtained, while if is too large (greater than about 4 K), unreasonably low Mesozoic values are obtained. This is illustrated on pages 80-83 of: "Berner, R.A., 2004. The Phanerozoic Carbon Cycle: CO2 and O2, Oxford University Press,Oxford, 150 pp". You might consider asking Bob Berner to draft a 5-10 line section, or incorporating the points and explanation given in my comments. [Danny Harvey (Reviewer's comment ID #: 101-57)]	Partly accepted, a short reference to pre-pleistocene findings on the relationship between CO2 and climate has been added to the introductory parapgraph of the section, referring to chapter 6 findings.
9-927	A	61:30	61:31	This sentence seems less certain than the statement on the lines 24-25 on 9-62. "a most likely value … between 1 and 4" versus "a most likely value around 3" [Brian Soden (Reviewer's comment ID #: 245-13)]	Accepted, sentence here and summary statement from chapter 9 only has been revised to synchronize.
9-928	A	61:39		after "observations", insert "as in Harvey and Kaufmann (2002)" [Danny Harvey (Reviewer's comment ID #: 101-58)]	Accepted.
9-929	A	61:42	61:45	(Same comment as for 9-60-28) The disagreement between Annan et al. and Schneider von Deimling et al. is not only due use of different models but also due to more realistic boundary conditions in the study by Schneider von Deimling et al.: in their study, LGM-specific dust & vegetation forcing is accounted for what is not the case in Annan et al. (as it is not in the PMIP-2 protocol). Schneider von Deimling et al. also estimated the magnitude of this effect in their publication: proper accounting for both boundary conditions shifts inferred climate sensitivity down by a value up to 1.4°C. Hence, the values derived by Annan et al. would have to be lowered by up to 1.5°C. Keeping this in	Accepted, text has been revised.

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				mind, the discrepancy between the two studies does not look that large any more (compare also Fig 9.6.1, p.9-119). Hence I strongly disagree that nothing can be learned from current LGM studies on climate sensitivity yet: Quite the contrary, they show that 3 independent sources (Annan et al., Schneider von Deimling et al.-tropics, Schneider von Deimling et al., Antarctica) are compatible with CS=1.5...4.5°C and strongly disagree with values above 5°C. [Hermann Held (Reviewer's comment ID #: 104-9)]	
9-930	A	61:48	61:48	The use of "traditional" here is not very specific. If it refers to the IPCC TAR range then that should be stated. [Gareth S. Jones (Reviewer's comment ID #: 121-99)]	Accepted, terminology has been revised.
9-931	A	62:4		typo:),s [Hermann Held (Reviewer's comment ID #: 104-10)]	Fixed
9-932	A	62:12		typo: C . [Hermann Held (Reviewer's comment ID #: 104-11)]	Fixed
9-933	A	62:15	62:26	Reading this section and the box in chapter 10, it is not clear what is the basis for raising the lower best estimate of climate sensitivity from 1.5 to 2.0. This conclusion is repeated many times in this report without a clear basis. Many of the studies referred to have significant probability densities between 1.5 and 2? Furthermore, 2 of the 7 model results that were the basis for the TAR estimates of future warming had a climate sensitivity below 2 (see TAR table 9.A1). Suggest explanation and revision of conclusion as appropriate. [Haroon Kheshgi (Reviewer's comment ID #: 125-37)]	Partly accepted, text has been clarified, and the overall assessment based on chapter 9 vs overall statement in ch10 has been more clarly separated. Revision of conclusion rejected, but justification for conclusion should now be clearer.
9-934	A	62:28		Para 9.7: this is a welcome and well written addition to Ch 9! [Fons Baede (Reviewer's comment ID #: 9-23)]	Noted, thanks.
9-935	A	62:30	62:30	Insert aftter "(see Sections 9.4,1-3)" "is most likely due to proximity of measuring equipment to human activities that are enjoying an increasing prosperity This is confirmed by the absence of evidence of temperature change in the " [VINCENT GRAY (Reviewer's comment ID #: 88-1353)]	Rejected. Incorrect on many grounds.
9-936	A	62:31	62:31	Insert after "atmosphere" "until the disturbances caused by the 1999 El Niño event" [VINCENT GRAY (Reviewer's comment ID #: 88-1354)]	Rejected. Reviewer now seems to be saying that the 1998 El Nino stopped the warming.
9-937	A	62:31	62:31	Replace "and ocean" by "There has been a large temperature increase in the ocean, but this appears to be periodic and may not represent an upwards trend." [VINCENT GRAY (Reviewer's comment ID #: 88-1355)]	Rejected. . Reviewer provides no justification for this change.
9-938	A	62:31	62:31	Delete "together with consistent evidence of" [VINCENT GRAY (Reviewer's comment ID #: 88-1356)]	Rejected. Reviewer provides no justification for this change.

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9-939	A	62:32	62:32	Replace "strengthen the overall evidence" by "do not confirm" [VINCENT GRAY (Reviewer's comment ID #: 88-1357)]	Rejected. Reviewer provides no justification for this change.
9-940	A	62:34	62:34	Replace "substantially stronger" by "no better" [VINCENT GRAY (Reviewer's comment ID #: 88-1358)]	Rejected. Evidence from several lines is stronger than evidence from just one line.
9-941	A	62:39	62:39	Replace " strong" with "That human activity on the surface, suchas building, energy emission and land-use change have had an influence on the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1359)]	Rejected. Available evidence does not support the reviewer's supposition.
9-942	A	62:40	63:40	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1360)]	Rejected. Style preference.
9-943	A	62:40	63:40	Insert after "included" "but they exclude important influences such as the El Niño effect and that of human activities on the surface" [VINCENT GRAY (Reviewer's comment ID #: 88-1361)]	Rejected. Available evidence does not support the reviewer's supposition that these influences are important for explaining the observed global mean warming..
9-944	A	62:40	63:41	"The" on line 40 to "reconstructions" on line 43. This is nonsense. How can "observed warming" be "highly significant"? [VINCENT GRAY (Reviewer's comment ID #: 88-1362)]	Rejected. Straightforward signfiicance testing.
9-945	A	62:43	62:43	"inhabited continents" is very odd wording. If what is meant by this is "all continents other than Antarctica" than this should be stated explicitly. [Michael Mann (Reviewer's comment ID #: 156-66)]	Accepted. Wording revised.
9-946	A	62:43	63:43	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1363)]	Rejected. Style preference.
9-947	A	62:43	63:43	Insert before "detectable" "is thought to be" [VINCENT GRAY (Reviewer's comment ID #: 88-1364)]	Rejected. Would misrepresent cited studies.
9-948	A	62:47	63:45	Replace from "It is highly unlikely (<5%)"in line 44 to "from" in line 45 with The highest recent temperature was the result of " [VINCENT GRAY (Reviewer's comment ID #: 88-1365)]	Rejected. Reviewer suggestion is out of place in this sentence.
9-949	A	62:47	63:2	The extravagant assertions in this section have not been substantiated and it sjould be deleted [VINCENT GRAY (Reviewer's comment ID #: 88-1366)]	Rejected. We are not aware of a "known mode of internal variability" that can lead to the observed widespread warming.
9-950	A	62:48	62:53	This argument is different from that in the exec summary, which is less compelling. [Michael Manton (Reviewer's comment ID #: 157-37)]	There is insufficient space in the ES to present the full argument, but wording there has been strengthened.
9-951	A	63:4	63:23	I can't go along with all this It has simply not been established	Rejected. Ocean heat content has

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				[VINCENT GRAY (Reviewer's comment ID #: 88-1367)]	increased. All elements of the cryosphere have indicated warming. So it has been established..
9-952	A	63:8		change "reduced" to "decreased" [Danny Harvey (Reviewer's comment ID #: 101-59)]	Accepted.
9-953	A	63:26	63:26	Insert after "surface" "as a result of increased human activity in the vicinity of the measuring equipment" [VINCENT GRAY (Reviewer's comment ID #: 88-1368)]	Rejected. This is not true.
9-954	A	66:20		add refs: " Chou, C. and J. D. Neelin, 2004: Mechanisms of global warming impacts on regional tropical precipitation. J. Climate, 17, 2688-2701." , "Chou, C., J. D. Neelin, U. Lohmann and J. Feichter, 2005: Local and remote impacts of aerosol climate forcing on tropical precipitation. J. Climate, 18, 4621-4636." and "Chou, C., J. D. Neelin, J.-Y. Tu, and C.-T. Chen, 2006: Regional tropical precipitation change mechanisms in ECHAM4/OPYC3 under global warming. J. Climate, in press." (accepted 12/05) [J. David Neelin (Reviewer's comment ID #: 187-21)]	References have been updated as appropriate.
9-955	A	69:41	69:41	Insert "Gray, V.R. 2000 "The Cause of Global Warming", Energy and Environmenmt. Vol 11, pages 613-529" [VINCENT GRAY (Reviewer's comment ID #: 88-1369)]	Rejected. No result that adds to chapter.
9-956	A	71:48	71:49	Correction to name. Should be "Jones, G.S."etc Reference should be:- Jones, G.S., A. Jones, D.L. Roberts, P.A. Stott, K.D. Williams, 2005: Sensitivity of global scale attribution results to inclusion of climatic response to black carbon. Geophysical Research Letters, 32, L14701, doi:10.1029/2005GL023370 [Gareth S. Jones (Reviewer's comment ID #: 121-100)]	Accepted. Reference corrected to include second initial.
9-957	A	74:34	74:34	Insert " McKitrick, R., % Michaels, P.J. 2004; "A test of corrections for extraneous signals in gridded surface temperature data. "Climate Research" Vol 26 pages 159-173." [VINCENT GRAY (Reviewer's comment ID #: 88-1370)]	Rejected. Chapter 3 is appropriate place if paper is worthwhile.
9-958	A	74:34	74:34	Insert "McIntyre, s., & McKitrick, R., 20003. "Corrections to the Mann et al (1998) proxy data base and northern hemispheric average temperature series",Energy and Environemnt. Vol 14, pages 751-771." [VINCENT GRAY (Reviewer's comment ID #: 88-1371)]	Rejected. Paleo chapter might be appropriate.
9-959	A	75:34		add ref: Neelin, J. D., C. Chou, and H. Su, 2003: Tropical drought regions in global warming and El Ni\~no teleconnections. Geophys. Res. Lett., 30(24), 2275, doi:10.1029/2003GL018625.	References updated as appropriate to reflect new citations.

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				[J. David Neelin (Reviewer's comment ID #: 187-22)]	
9-960	A	76:54	76:54	Insert "Peterson, T.C. 2003: "Assessment of Urban Versus Zrural in Situ Surface Temperatures in the contiguous Unitd states. Journal of Climate Vol 16. pages 2939-2959" [VINCENT GRAY (Reviewer's comment ID #: 88-1372)]	Rejected. Chapter 3 is appropriate place.
9-961	A	78:52		accepted -> DOI 10.1007/s00382-006-0126-8 [Hermann Held (Reviewer's comment ID #: 104-13)]	Accepted.
9-962	A	80:11		"Ffye" to "Fyfe" [Daithi Stone (Reviewer's comment ID #: 256-14)]	Accepted.
9-963	A	83:23	83:23	Insert "Zhao,Z., Y. Ding, Y Luo, & S Wang; Recent Studies on attributions of climate change in China" Acta Meteorologica Sinica, Vol 19. pages 389-400" [VINCENT GRAY (Reviewer's comment ID #: 88-1373)]	Rejected. Chapter 3 might be appropriate.
9-964	A	84:0		Should tone down the negativity in the first sentence, otherwise it raises the question of why this question is even being put forward? [Govt. of United States of America (Reviewer's comment ID #: 2023-653)]	Rejected. Important to say, right off, that individual events can not be linked to anthropogenic forcing. Saying so does not invalidate the question. See 9-967
9-965	A	84:1	84:1	Delete "by Climate Change" You are now using the forbidden definition of the FCCC [VINCENT GRAY (Reviewer's comment ID #: 88-1374)]	Rejected. However, text revised to refer specifically to anthropogenic greenhouse gas emissions.
9-966	A	84:1	86:42	These two sections are utterly repetitive of what has already been said over and over again. Do we really need it sio many times!. Delete them [VINCENT GRAY (Reviewer's comment ID #: 88-1455)]	Rejected.. The section in question are the 2 FAQs, that are written for a different audience than the main chapter body.
9-967	A	84:3		Suggest stronger opening sentence: 'Attributing a specific, single extreme event to a specific cause is difficult, if not impossible, because several factors usually need to combine to produce an extreme event.' [David Wratt & David Fahey (Reviewer's comment ID #: 67-64)]	Accepted. Thank you.
9-968	A	84:4	84:4	Delete "difficult if not" [VINCENT GRAY (Reviewer's comment ID #: 88-1375)]	Rejected.. We do not wish to rule out a mechanistic explanation for individual extreme events by using impossible
9-969	A	84:4	84:4	Change "As well" to "Also". [David Parker (Reviewer's comment ID #: 195-137)]	Accepted.
9-970	A	84:5	84:5	Sentence should read: Nevertheless the human-induced … The style manual by Strunk and White say you should not start a sentence with the word however, but you can use nevertheless to start a sentence.	Accepted.

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				[Wilmer Anderson (Reviewer's comment ID #: 5-43)]	
9-971	A	84:5	84:6	Dlete from "However" on ;ine 5 to "likellihood of" You have not established that there is any unusual "warming" [VINCENT GRAY (Reviewer's comment ID #: 88-1376)]	Rejected. This was done even in the TAR.
9-972	A	84:6	84:6	Delete "that" [VINCENT GRAY (Reviewer's comment ID #: 88-1377)]	Rejected. Style preference.
9-973	A	84:6	84:7	FAQ 9.1: "…could have increased …" and " … could have decreased …" These seem a bit weak. Are the instances of "could" necessary here, especially given the words "suggests that the likelihood of" indicate uncertainty? It seems that the content of an FAQ might be a bit more straightforward. [Melinda Marquis (Reviewer's comment ID #: 162-78)]	Accepted.
9-974	A	84:10	84:10	Replace "greater" by "overwhelming" [VINCENT GRAY (Reviewer's comment ID #: 88-1378)]	Rejected. Reviewer provides no justification for this change.
9-975	A	84:15	84:15	Replace "climate change" with "changes the cllimate" [VINCENT GRAY (Reviewer's comment ID #: 88-1379)]	Rejected. Style preference.
9-976	A	84:22	84:22	Sentence should read: … so it is not possible … [Wilmer Anderson (Reviewer's comment ID #: 5-44)]	No longer relevant – see 9-978.
9-977	A	84:22	84:22	Change "would be" to "is". [David Parker (Reviewer's comment ID #: 195-138)]	No longer relevant – see 9-978.
9-978	A	84:22		Suggest deleting this sentence as repetitive and start with next sentence: 'Extreme weather results…' [David Wratt & David Fahey (Reviewer's comment ID #: 67-65)]	Accepted.
9-979	A	84:23	84:23	Change "As well" to "Also". [David Parker (Reviewer's comment ID #: 195-139)]	See 9-978
9-980	A	84:28	84:30	Amend sentence to "Because some factors (e.g., sea surface temperatures) may be strongly affected by human activities but others may not, it is not simple to detect a human influence on a single, specific extreme event." [David Parker (Reviewer's comment ID #: 195-140)]	Accepted.
9-981	A	84:29	84:29	Sentence should read: … this complicates the detection … [Wilmer Anderson (Reviewer's comment ID #: 5-45)]	See 9-980.
9-982	A	84:32	84:32	Sentence should read: Nevertheless we may able … The style manual by Strunk and White say you should not start a sentence with the word however, but you can use nevertheless to start a sentence. [Wilmer Anderson (Reviewer's comment ID #: 5-46)]	Accepted.
9-983	A	84:32	84:32	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1380)]	Rejected. Style preference.

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9-984	A	84:36	84:36	Sentence should read: Such models indicate …The word experiments and models should not be confused. Computer models are not experiments but are based on experimental data. [Wilmer Anderson (Reviewer's comment ID #: 5-47)]	Accepted.
9-985	A	84:43	84:43	Sentence should read: Nevertheless careful analysis … The style manual by Strunk and White say you should not start a sentence with the word however, but you can use nevertheless to start a sentence. [Wilmer Anderson (Reviewer's comment ID #: 5-48)]	Accepted.
9-986	A	84:45	84:45	Replace "variability" with "other climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1381)]	Rejected. Reviewer has confused variability with variations.
9-987	A	84:45		Suggest for clarity changing to 'natural variability' [David Wratt & David Fahey (Reviewer's comment ID #: 67-66)]	Disagree – I dont think this is the meaning we are trying to suggest here. Sentence now refers to change in climate variability. Such a change might have natural or anthropogenic origins.
9-988	A	84:46	84:46	Replace "variability" with "other climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1382)]	Rejected. See 9-986.
9-989	A	84:47	84:47	Replace "climate variability" by "all the many other climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1383)]	Rejected. See 9-986.
9-990	A	84:49	84:49	Sentence should read: …approach can be adopted to examine … [Wilmer Anderson (Reviewer's comment ID #: 5-49)]	Accepted.
9-991	A	84:53	84:53	Sentence should read: Nevertheless there is as yet no ...The style manual by Strunk and White says you should not start a sentence with the word however, but you can use nevertheless to start a sentence. [Wilmer Anderson (Reviewer's comment ID #: 5-50)]	Sentence revised.
9-992	A	85:0		Since this is supposed to be new since the TAR, why not phrase question 2: why has warming accelerated since the 1970s? [Govt. of United States of America (Reviewer's comment ID #: 2023-654)]	Interesting question but not our (Chapter 9) call
9-993	A	85:1	85:1	Replace "variability" by "climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1384)]	Rejected. This change would simply ask if a climate change could be explained by a climate change.
9-994	A	85:3	85:3	Replace "over the last centiury" with "" shown only by the surface record, from 1978, but not between 1942 and 1978" [VINCENT GRAY (Reviewer's comment ID #: 88-1385)]	Rejected. The surface temperature has warmed since the late 19th century.
9-995	A	85:3		Suggest starting this response with a direct answer, namely by repeating thought in ln16-	Accepted.

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				17: 'It is very unlikely that the 20th century warming can be explained by natural variability. The warming has been very unusual….' [David Wratt & David Fahey (Reviewer's comment ID #: 67-68)]	
9-996	A	85:3		This answer is very complicated for the non-expert reader. It could be reduced in length by at least a third and focused better on the answer. The style is very assertive rather than explanatory. For example ln 27-29, 'The combined effects of natural internal variability and natural external forcings can also be estimated from paleoclimatic reconstructions of pre-industrial climate variations' will be a difficult thought for the non-expert reader and offers no help to the reader as to 'why' or 'why is this important'. [David Wratt & David Fahey (Reviewer's comment ID #: 67-69)]	Accepted. Text revised and shortend.
9-997	A	85:3	:18	The wording of this paragraph suggests that natural factors have had no influence on 20th century warming. What should be made clear is that natural factors ALONE cannot explain the warming. This should be made clear in lines 7 and 17. [Joanna Haigh (Reviewer's comment ID #: 95-13)]	Accepted. Text revised.
9-998	A	85:6	85:5	Insert after "concentration", "but is much more likely to reflect better living conditions in human activities near the measuring equipment" [VINCENT GRAY (Reviewer's comment ID #: 88-1386)]	Rejected. No evidence that this assertion is true.
9-999	A	85:6	85:6	Replace "and" with."It is" [VINCENT GRAY (Reviewer's comment ID #: 88-1387)]	Rejected. Style preference.
9-1000	A	85:7	85:7	Insert "many" after from" [VINCENT GRAY (Reviewer's comment ID #: 88-1388)]	Rejected. Text revised to refer to known natural forcing factors.
9-1001	A	85:8	85:8	Insert after "activity)' "but the most prominent feature, the maximum temperature shown, in 1999, is attributable to the unusually strong El Niño event of that year" [VINCENT GRAY (Reviewer's comment ID #: 88-1389)]	Rejected. Such a statement is confusing and of limited relevance to warming over a century.
9-1002	A	85:8	85:8	Delete "good" [VINCENT GRAY (Reviewer's comment ID #: 88-1390)]	Rejected. Reviewer provides no justification for this change.
9-1003	A	85:9	85:9	Insert after "include "several of" [VINCENT GRAY (Reviewer's comment ID #: 88-1391)]	Rejected. Reviewer does not provide justification for proposed change.
9-1004	A	85:9	85:9	Delete "dominant" [VINCENT GRAY (Reviewer's comment ID #: 88-1392)]	Rejected. Reviewer does not provide justification for proposed change.
9-1005	A	85:10	85:10	Insert after "but" "one of the most important, the El Niño and La Niña ocean effects. Have been omitted, despite their obvious influence, and the local effect due to the proximity of measuring equipment to human activity, which have been shown to be significant in a recent statistical study, have also been omitted so it is not surprising that it"" [VINCENT GRAY (Reviewer's comment ID #: 88-1393)]	Rejected. The possibility of warming trends arising from these factors have been considered.
9-1006	A	85:10	85:10	Replace "fail" by "fails"	Rejected. The change would make the

No.	Batch	Page:line		Comment	Notes
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				[VINCENT GRAY (Reviewer's comment ID #: 88-1394)]	sentence ungrammatical
9-1007	A	85:13	85:13	Replace "the most important patterns of climate varuiabilitysuch assuch as"" with "some climate influences, but not"" [VINCENT GRAY (Reviewer's comment ID #: 88-1395)]	Rejected. The change would make the sentence wrong.
9-1008	A	85:14	85:14	Insert after "ENSO)" "which was always evident, particularly with the high temperature poeak of 1999, and aslo, the local human effects on the surface have been neglected" [VINCENT GRAY (Reviewer's comment ID #: 88-1396)]	Rejected. The possible effects of these factors have not been disregarded and this is evident from the description.
9-1009	A	85:14	85:14	FAQ 9.2: "Climate models *almost never* simulate …" Could this be rephrased to avoid the "almost never," which I believe is present because of one study only? Could this be re-worded to be more straightforward, especially given that this is in an FAQ? [Melinda Marquis (Reviewer's comment ID #: 162-79)]	Accepted. This sentence has been worred more directly..
9-1010	A	85:15	85:15	Replace "external" with "deliberately restricted" [VINCENT GRAY (Reviewer's comment ID #: 88-1397)]	Rejected. This is not correct.
9-1011	A	85:16	85:16	Replace "various" with "highly distorted" [VINCENT GRAY (Reviewer's comment ID #: 88-1398)]	Rejected. Reviewer provides no justification for this change.
9-1012	A	85:16	85:17	FAQ 9.2 says that it's '*very* unlikely that the 20th century warming can be explained by natural variability," but the SPM (page 10, lines 25-27) makes a similar statement -- about global climate change in the last 50 years -- but refers to it as "*highly* unlikely." Is the distinction between "very unlikely" and "highly unlikely" intentional? If so, is this because the former refers to the entire 20th century, whereas the latter refers only to the last 50 years? If so, this intentional distinction and greater degree of (un)likelihood should perhaps be stated explicitly. [Melinda Marquis (Reviewer's comment ID #: 162-91)]	Well spotted. Q9.2 was correct. An inconsistency had crept into the ES, which is now corrected.
9-1013	A	85:16	85:16	Change "very" to "highly"? [David Parker (Reviewer's comment ID #: 195-141)]	See 9-1012.
9-1014	A	85:17	85:17	Replace "20th century warming" with 'the restricted warming period since 1978 as shown only by the surface record" [VINCENT GRAY (Reviewer's comment ID #: 88-1399)]	Rejected. This is not correct. Warming started in late 19th century.
9-1015	A	85:17	85:18	Replace from "natural variability on line 17 to ""activity)' on line 18 with "human-induced forcing effects" [VINCENT GRAY (Reviewer's comment ID #: 88-1400)]	Rejected. Reviewer provides no justification for this change.
9-1016	A	85:20	85:20	Replace "natural internal processes" with "many natural and human influeces near the surface" [VINCENT GRAY (Reviewer's comment ID #: 88-1401)]	Rejected. Reviewer provides no justification for this change.
9-1017	A	85:20	85:29	Insert after "ENSO" "and human influences such as building of cities, energy emission and land-use changes"	Rejected. Such factors are included already in this sentence.

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				[VINCENT GRAY (Reviewer's comment ID #: 88-1402)]	
9-1018	A	85:24	85:24	Insert after "of" "a deliberately restricted number of" [VINCENT GRAY (Reviewer's comment ID #: 88-1403)]	Rejected. Reviewer provides no justification for this change.
9-1019	A	85:24	85:24	Replace "naturtal internal processes" with "a whole range of miscellaneous climate influences"" [VINCENT GRAY (Reviewer's comment ID #: 88-1404)]	Rejected. No, this cannot be done in the way the reviewer seems to believe it can.
9-1020	A	85:27	85:27	Replace "natural internal variability" with ."miscellaneous climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1405)]	Rejected. Reviewer provides no justification for this change. Reviewer does not specify what "miscellaneous climate influences" might be at play.
9-1021	A	85:31	85:31	Replace "over the 20th century" by "in the surface record since 1856" [VINCENT GRAY (Reviewer's comment ID #: 88-1406)]	Rejected. This change would produce a statement that would be somewhat too confident of how well models do in reproducing the late 19th century climate variations.
9-1022	A	85:33	85:33	Replace "external forcing" with climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1407)]	Rejected. These are accurately described as external forcings.
9-1023	A	85:33	85:33	Replace "external forcing factors" with "climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1408)]	Rejected. See 9-1023.
9-1024	A	85:33	85:33	Insert after "aersols" "the El Niño ocean events," [VINCENT GRAY (Reviewer's comment ID #: 88-1409)]	Rejected. El Nino is not an external forcing.
9-1025	A	85:33	85:34	Delete sentence "Natural….output" which effectively repeats lines 21-22. [David Parker (Reviewer's comment ID #: 195-142)]	Accepted.
9-1026	A	85:36	85:36	Insert after "variations" "El Niño causes an observable temperature rise, particularly prominant in 1999, and its counterpart, La Niña, a temperature fall". [VINCENT GRAY (Reviewer's comment ID #: 88-1410)]	Rejected. This description is about long term changes, not the short time scale associated with El Nino.
9-1027	A	85:37	85:37	Replace "a rapid" by "an" [VINCENT GRAY (Reviewer's comment ID #: 88-1411)]	Rejected. A ~30% increase in 100 years is rapid.
9-1028	A	85:37	85:37	Delete "well-mixed" They are NOT "well-mixed" [VINCENT GRAY (Reviewer's comment ID #: 88-1412)]	Rejected. Standard terminology.
9-1029	A	85:38	85:38	Replace renained at near stable" with "had smaller" [VINCENT GRAY (Reviewer's comment ID #: 88-1413)]	Rejected. Reviewer provides no justification for this change.
9-1030	A	85:40	85:40	Adsdcat end "Human actibity on the surface, such as the building of cities, the emission of energy, and land-use changes, has had a major influence in the climate, reflected in the surface temperature record" [VINCENT GRAY (Reviewer's comment ID #: 88-1414)]	Rejected. Reviewer provides no justification for this change. There is no evidence that cities have caused the ocean heat content to increase.

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9-1031	A	85:44	85:44	Replace "external forcing" by"the building of cities, the emission of energy, and land-use changes, has had a major influence in the climate, reflected in the surface temperature record" [VINCENT GRAY (Reviewer's comment ID #: 88-1415)]	Rejected. Excludes natural external forcings as well as the more important anthropogenic forcings.
9-1032	A	85:46	85:46	Insert after "before" "weather stations were surrounded by buildings, cars or aircraft", [VINCENT GRAY (Reviewer's comment ID #: 88-1416)]	Rejected. Glaciers, oceans, and rural stations show warming and are not surrounded by buildings, cars or aircraft.
9-1033	A	85:46	85:47	Glad to see you mentioned the likely role of tall stacks in lofting sulfate precursors and lengthening sulfate aerosol lifetimes. I think this point will need to later be more fully developed in models and analyses. [Michael MacCracken (Reviewer's comment ID #: 152-266)]	Given that this aspect is not really elaborated on in chapter 9, and would need more explanation, it has been deleted.
9-1034	A	85:48	85:48	Replave "levelling off" with "fall" [VINCENT GRAY (Reviewer's comment ID #: 88-1417)]	Rejected. The description is accurate.
9-1035	A	85:48	85:48	Insert after "temperatures" ."of the surface" [VINCENT GRAY (Reviewer's comment ID #: 88-1418)]	Accepted.
9-1036	A	85:48	85:48	Replace "in the 1950s and 1960s" with ":from 1942 to 1978" [VINCENT GRAY (Reviewer's comment ID #: 88-1419)]	Rejected. Why so detailed?
9-1037	A	85:48	85:48	Add at end "a move of weather stations to the early airports, [VINCENT GRAY (Reviewer's comment ID #: 88-1420)]	Rejected. Levelling off was also observed in stations that did not move to airports.
9-1038	A	85:52	85:52	Replace "greenhouse gas forcings has " with "improved huiman living conditions have" [VINCENT GRAY (Reviewer's comment ID #: 88-1421)]	Rejected. Reviewer provides no justification for this change.
9-1039	A	85:52	85:52	Replace "forcings" .by "other climatre influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1422)]	Rejected. The reviewer wants a stronger statement here than is warranted by the evidence.
9-1040	A	85:54	85:54	Insert after "warming" "in the surface record" [VINCENT GRAY (Reviewer's comment ID #: 88-1423)]	Rejected. Also seen away from surface.
9-1041	A	85:54	85:54	Replace "four" by "two" [VINCENT GRAY (Reviewer's comment ID #: 88-1424)]	Rejected. Warming in lower atmosphere over four decades.
9-1042	A	85:54	85:54	Replace "consistent" with "inconsistent" [VINCENT GRAY (Reviewer's comment ID #: 88-1425)]	Rejected. Reviewer provides no justification for this change.
9-1043	A	85:55	85:55	Insert after "forcing" "because the temperature fell since 1942, and there is no evidence of such an efgfect inn the lower troposphere, where it is supposed to be prominent" [VINCENT GRAY (Reviewer's comment ID #: 88-1426)]	Rejected. Reviewer provides no justification for this change.
9-1044	A	85:55	85:55	Insert after "(the troposphere)" " but only if the single El Niño peak of 1999 is taken to	Rejected. Warming even if 1998 is

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				represent a warming trend, after consistent evidence showing no warming since 1958 (for radiosondes) and 1978 (for satellites)" [VINCENT GRAY (Reviewer's comment ID #: 88-1427)]	excluded.
9-1045	A	85:55	85:55	Replace "and" by "there was also" [VINCENT GRAY (Reviewer's comment ID #: 88-1428)]	Rejected. Style preference.
9-1046	A	85:56	85:56	Delete "Also" [VINCENT GRAY (Reviewer's comment ID #: 88-1429)]	Rejected. Reviewer provides no justification for this change.
9-1047	A	85:57	86:2	Replace from "Such patterns" on page 85 line 57 to (ENSO) on pahe 86 line 2 with "Such patterns are consistent with the bether living conditions in developed countries, and particularly, the better heating in the winter" [VINCENT GRAY (Reviewer's comment ID #: 88-1430)]	Rejected. Reviewer provides no justification for this change.
9-1048	A	86:5	86:5	Add at end "but with little success" [VINCENT GRAY (Reviewer's comment ID #: 88-1431)]	Rejected. Mis-represents the science.
9-1049	A	86:7	86:7	Insert after "only" "a deliberately restricted number of" [VINCENT GRAY (Reviewer's comment ID #: 88-1432)]	Rejected. Sentence is clear as written.
9-1050	A	86:8	86:8	Sentence should read: Nevertheless , as shown … The style manual by Strunk and White says you should not start a sentence with the word however, but you can use nevertheless to start a sentence. [Wilmer Anderson (Reviewer's comment ID #: 5-51)]	Accepted.
9-1051	A	86:8	86:8	Insert after "aerosols" "but not El Niño effects, or the important local influences of human urban and land-use developments" [VINCENT GRAY (Reviewer's comment ID #: 88-1433)]	Rejected. Reviewer provides no justification for this change.
9-1052	A	86:10	86:10	Delete "all the most important forcings including" [VINCENT GRAY (Reviewer's comment ID #: 88-1434)]	Rejected. Reviewer provides no justification for this change.
9-1053	A	86:11	86:11	(nsert after "aerosols)" "but not the important local influences of human urban and land-use developments" [VINCENT GRAY (Reviewer's comment ID #: 88-1435)]	Rejected. No justification for supposing that these would have any important global effect. Much evidence to the contrary.
9-1054	A	86:11	86:11	Replace "natural external forcinsgs" by "some climate influences, but not including the important El Niño" [VINCENT GRAY (Reviewer's comment ID #: 88-1436)]	Rejected. It is implausible that a natural internal process, El Nino, would cause a century-long warming trend.
9-1055	A	86:12	86:12	"are" "claimed to be" [VINCENT GRAY (Reviewer's comment ID #: 88-1437)]	Rejected. Would misrepresent the science.
9-1056	A	86:13	86:13	Replace "forcing" by local effects due to urban development and land-use change" [VINCENT GRAY (Reviewer's comment ID #: 88-1438)]	Rejected. Reviewer provides no justification for this change.
9-1057	A	86:13	86:14	FAQ 9.2 states that "The effect of human-induced forcing *very likely* dominates over	Noted. FAQ 9,2 is consistent with the

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				all other causes of *global* mean surface temperature change during the past half century." Is this consistent with the SPM (page 11, lines 32-35), where it states that it is *likely* that GHG forcing has been the dominant cause of the observed warming of the *northern hemisphere* over the last 50 years? [Melinda Marquis (Reviewer's comment ID #: 162-92)]	Ch 9 text and ES. The July 28 version of the SPM also appears to be consistent.
9-1058	A	86:13		It may be worth mentioning that we hardly have any observations from Antarctica. [Daithi Stone (Reviewer's comment ID #: 256-15)]	Accepted.
9-1059	A	86:15	86:15	Replace "variability" by "changes" [VINCENT GRAY (Reviewer's comment ID #: 88-1439)]	Rejected. Reviewer confuses variability and variations.
9-1060	A	86:16	86:17	Replace "internal ckimate variability" by "various climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1440)]	Rejected. The model runs referred to here are about internal variability only.
9-1061	A	86:18	86:18	Replace "internal climate mechanisms" by miscellaneous climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1441)]	Rejected. See 9-1060
9-1062	A	86:18	86:18	Addc at end "but urban and land-use change does" [VINCENT GRAY (Reviewer's comment ID #: 88-1442)]	Rejected. Reviewer provides no justification for this change.
9-1063	A	86:22	86:22	Add at end "as" [VINCENT GRAY (Reviewer's comment ID #: 88-1443)]	Accepted. Thanks.
9-1064	A	86:24	86:24	Delete "common" [VINCENT GRAY (Reviewer's comment ID #: 88-1444)]	Rejected. Reviewer provides no justification for this change.
9-1065	A	86:25	85:25	Replace "and cooling" with "but do predict" [VINCENT GRAY (Reviewer's comment ID #: 88-1446)]	Rejected. Reviewer provides no justification for this change.
9-1066	A	86:25	86:25	Insert aftre "ocean" ", they wrongly predict" [VINCENT GRAY (Reviewer's comment ID #: 88-1445)]	Rejected. Reviewer provides no justification for this change.
9-1067	A	86:26	86:26	Replace "help" by "fail" [VINCENT GRAY (Reviewer's comment ID #: 88-1447)]	Rejected. Reviewer provides no justification for this change.
9-1068	A	86:30	86:30	Replace "forcing' by "local effects caused by urbanisation and land-usgae changes" [VINCENT GRAY (Reviewer's comment ID #: 88-1448)]	Rejected. Reviewer provides no justification for this change.
9-1069	A	86:33	86:43	This paragraph lacks focus. It should be made clear whether this paragraph is using the millennia reconstructions to test long term trends, climate forcing versus response, or neither. [Govt. of Australia (Reviewer's comment ID #: 2001-371)]	Accepted, focus of paragraph sharpened..
9-1070	A	86:35	86:35	Replace "natural internal variability and natural external forcings" by a variety of miscellaneous climate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1449)]	Rejected. Good to remind readers that internal and external natural processes can affect climate.
9-1071	A	86:36	86:36	Replacve "some" by "considerable" [VINCENT GRAY (Reviewer's comment ID #: 88-1450)]	Rejected. Reviewer provides no justification for this change.

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9-1072	A	86:36	86:36	Delete "variability" [VINCENT GRAY (Reviewer's comment ID #: 88-1451)]	Rejected. Nevertheless, "variability" replaced with "variations".
9-1073	A	86:37	86:38	FAQ 9.2: This sentence says that reconstructions indicate that warming in the 20th century is stronger than other warming of last *1,000* years. Should this be 1,000 years or 700 years? I am confused because Section 9.3.3.1 (page 19, line 49) refers to 1,000 years, and page 20 (lines 3-5) refer to "last millenium." However, the Ch. 9 Ex. Sum. (page 4, lines 18-21) refer to "reconstructions of the past 7 centuries." Further, doesn't the Hegerl (2006, Nature) paper refer to reconstructions of the past seven centuries, but when it's referred to on page 59 (lines 30-31), it appears to be used to refer to reconstructions for the "past millenium." I'm just checking on the consistency of 700 years vs. 1, 000 years in all relevant places. [WG1 TSU (Reviewer's comment ID #: 285-13)]	Wording made consistent with Ch 6 ES (TOD version). The Ch 6 assessment is consistent with the Ch 9 assessment regarding the role of external forcing during the past 7 centuries.
9-1074	A	86:38	86:38	Insert after "1000 years" "and is due to human activity on the earth's surface such as urban development, energy emissions and land-use change" [VINCENT GRAY (Reviewer's comment ID #: 88-1452)]	Rejected. Reviewer provides no justification for this change.
9-1075	A	86:40	86:40	Insert after "forcing" " and some as a result of errors in calculation" [VINCENT GRAY (Reviewer's comment ID #: 88-1453)]	Rejected. Reviewer provides no justification for this change.
9-1076	A	86:42	86:42	Replace "forcings" by" human activity on the earth's surface such as urban development, energy emissions and land-use change " [VINCENT GRAY (Reviewer's comment ID #: 88-1454)]	Rejected. Reviewer provides no justification for this change.
9-1077	A	89:0	89:	The climate is never in equilibrium so thjis Table never applies [VINCENT GRAY (Reviewer's comment ID #: 88-1456)]	Rejected. Standard terminology and framing of question.
9-1078	A	90:0	90:	Spelling in Table 9.6.1., second last study, 4th column: Insulation -> Insolation [Hermann Held (Reviewer's comment ID #: 104-18)]	Accepted.
9-1079	A	91:0	91:	So you repeat the whole lot once again, in a form incapable of amendment> I suppose you think that if it is indefinitely repeated somebody might believe it [VINCENT GRAY (Reviewer's comment ID #: 88-1457)]	Noted.
9-1080	A	91:7		I find the statements in the second and third (non title) rows hard to reconcile. It seems to suggest that if greenhouse gas forcing has been involved then it has to be dominant, ie there is no room for it to be a minor contributor. [Daithi Stone (Reviewer's comment ID #: 256-16)]	Noted. We don't quite see the difficulty. The assessment is that greenhouse gas has been the dominant contributor.
9-1081	A	95:0	96:	These pages are to divert attention from your statistical misdenmenours, which are : The derivation nof averages from non-representative samples- most evident with the surface temperature record; The dfrawing of straight lines through irregular or periodic sequences; also with the surface record, and with the ocean heat record; and total neglect of bias assessments in practically all of your figures.	Noted.

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				[VINCENT GRAY (Reviewer's comment ID #: 88-1458)]	
9-1082	A	95:0		Appendix 9.A Several times in this section "CGCM" is used. What is a CGCM? In earlier parts of this chapter it seems to mean a particular name for a model from the Canadian Centre for Climate Modelling and Analysis (CCCma) e.g. CGCM2. But I think here it is a generic term for a type of model (Coupled Global Climate Models). Is this defined anywhere? [Gareth S. Jones (Reviewer's comment ID #: 121-103)]	Accepted. We now use the standard acrnomyn AOGCM throughout.
9-1083	A	97:27	97:29	AOGCM is used here but in the previous appendix 9.A, CGCM is used. Is the difference delibrate? [Gareth S. Jones (Reviewer's comment ID #: 121-104)]	Terminology has been synchronized.
9-1084	A	97:48	97:51	As stated above, a uniform prior does not merely represent bounds, but a very specific distribution between those bounds. There is no such thing as an "ignorant prior". [James Annan (Reviewer's comment ID #: 6-11)]	Noted. No change necessary
9-1085	A	97:51	98:2	Such upper and lower bounds are not merely "somewhat subjective", they are essentially subjective. This does not mean that they are not based on transparent reasoning. [Paul Baer (Reviewer's comment ID #: 10-9)]	Accepted.
9-1086	A	98:20	98:22	It is not merely difficult, it is impossible to generate demonstrably independent priors that are not essentially arbitrary (like typical uniform priors). This problem has been much discussed within the literature on Bayesian statistics and has not been effectively resolved. [Paul Baer (Reviewer's comment ID #: 10-8)]	Accepted.
9-1087	A	100:0	100:	These graphs are based on the false assumption that temperature changes can be properly represented by a linear trend. All of the influences mentioned are highly irregular. Also you leave out an important influence, the El Niño ocean events, let alone the surface effects due to humans such as urbanisation and land-use change [VINCENT GRAY (Reviewer's comment ID #: 88-1459)]	Rejected. Linear trend is an accepted way of presenting changes over time.
9-1088	A	101:0	101:	The value or importance of these graphs is not explained. The earth is never in equilibrium and they seem to be irrelevant [VINCENT GRAY (Reviewer's comment ID #: 88-1460)]	Rejected. These graphs are discussed on 9-13 and 9-14.
9-1089	A	102:5	102:12	Figure 9.2.3: This graph has presumably been created especially for the IPCC 4th report. If so more details must be given about how it was created. If this had been produced from a paper then hopefully there would be full details about what processing was done, here there are very few details, but I suspect quite a bit of processing went into it. For instance does the observed data have any missing data temporally or spatially? Was a missing data mask also applied to the model data? Were the model data really anomalised with respect to the whole period (as stated)? The data points either side of the Pinatubo spike are suspiciously close to zero, suggesting the +ve values around Pinatubo have not been included in the period mean.	Accepted. Additional details provided in the on-line supplementary information. See Appendix 9.C.

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				[Gareth S. Jones (Reviewer's comment ID #: 121-106)]	
9-1090	A	102:7	102:10	Why are not all models given in table 8.2.1 included here? In particular HadCM3 and HadGEM1(ids 22 and 23)? Data from 20th Century runs from both models are available on the IPCC 4th Assessment Model Output database. At the very least should explain the reasons why any models are being excluded. [Gareth S. Jones (Reviewer's comment ID #: 121-105)]	Accepted. The models plotted in Figure 9.2.3 were those available with all the requisite data in June 2005 (when the analysis was performed. This is now stated in the on-line supplementary information associated with this figure.
9-1091	A	103:0	103:	These simulations are incomplete. They omit El Niño ocean effects and they ignore completely the human-induced surface effects, such as urban development, energy emission and land-use change. [VINCENT GRAY (Reviewer's comment ID #: 88-1461)]	Rejected. Figure is smoothed to remove short-term variability. Models largely reproduce observed changes without urban development and land-use change indicating these effects are unlikely to be major influences on hemispheric scale.
9-1092	A	103:0		Figure 9.3.1: This figure is not very clear. The keys in the figure do not mention all the lines available e.g. what is the green line in the top plot (in the text it says instrumental data). Some of the colours are conflicting (e.g. Dark blue is Briffa in top but volcanic in bottom), and there appears to be no mention about what the two coloured shaded regions are (yellow and light blue) is it the 90% uncertainty mentioned in the label? I am sure the authors understand what is happening here, but I suggest some cosmetic changes for the readers benefit to improve clarity. [Gareth S. Jones (Reviewer's comment ID #: 121-107)]	Partially accepted. Caption revised. Figure remains unchanged because we reproduce a figure from the published literatures.
9-1093	A	104:0	104:	These simulations are incomplete. They omit El Niño ocean effects and they ignore completely the human-induced surface effects, such as urban development, energy emission and land-use change. Also I cannot believe that volcanoes and solar effects can realistically simulate the temperature rise from 1910 to 1942 in the surface record [VINCENT GRAY (Reviewer's comment ID #: 88-1462)]	Rejected. Models include El Nino. Models largely reproduce longterm observed changes without urban development and land-use change indicating these factors are unlikely to be major influences on global scale. Nevertheless some of these models do include this effect. Reviewer provides no evidence or reasoning to support his lack of belief.
9-1094	A	104:0		Figure 9.4.1. Use HadCRUT3 (Brohan et al., 2006) for the observations. Tidy up caption from line 8 onwards to: "The 51 simulations that include both anthropogenic and natural forcings came from 13 models, 11 of which are described in Chapter 8, Table 8.2.1 and are identified as follows by their Model ID given in that table: CCSM3 ... [omitting GFDL R30 and omitting 6:, 7: etc] ...HadCM3 (22,4). An additional two.....(Delworth et al.,	Accepted. HadCRUT3 now used. Caption improved.

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				2002). The 19 model simulations...HadCM3 (22,4). The observed and simulated..... the 1901-1997 mean. The simulations in (b) are expressed as anomalies relative to the 1901-1997 climatology of the corresponding model simulation that also includes anthropogenic forcing. Only models.....1996 to the end of the available runs. The observations were centered relative to 1901-1997" The NAT simulations in the lower panel are not extended to 2005 despite the caption. [David Parker (Reviewer's comment ID #: 195-143)]	
9-1095	A	104:4	104:25	This caption is confusing, in particular lines 17 and 19. This caption, as well as many other captions in this chapter, is much too long. The essential information is difficult to find. [Fons Baede (Reviewer's comment ID #: 9-24)]	Partly accepted. Caption improved (see 9-1094), and some details have been shifted to electronic supplement (see Appendix 9.C). However, completeness is more important than shortness.
9-1096	A	104:4	104:25	Figure 9.4.1: It looks like this graph has been created especially for this report. If so more details should be given about what processing was done. For instance the observations has missing spatial data, was a mask of this missing data applied to the model simulations for annual or monthly data? Was the temperatures calculated with respect to the global mean of the 1901-1997 period or was a global mean calculated after the anomaly was calculated on each gridpoint with respect to that gridpoint 1901-1997 average? I am concerned that the methods that go into such plots as this will have less rigorous examination than that from a peer-reviewed publication which would have hopefully fully shown what was done. [Gareth S. Jones (Reviewer's comment ID #: 121-110)]	Accepted. Details provided in electronic supplement. See Appendix 9.C.
9-1097	A	104:11	104:14	Why are not all models given in table 8.2.1 included here? In particular HadGEM1(id 23)? Data from 20th Century runs from the model, which includes natural and anthropogenic forcing, are available on the IPCC 4th Assessment Model Output database.The control has a trend of less than 0.2K/century, so that cannot be the reason. [Gareth S. Jones (Reviewer's comment ID #: 121-108)]	All models available at the time the figure was prepared are included. The criteria for inclusion are explained in the supplementary information (Appendix 9.C).
9-1098	A	104:13	104:14	Halfway through the list of models, a numbering system suddenly appears. [Daithi Stone (Reviewer's comment ID #: 256-17)]	Caption revised.
9-1099	A	104:20	104:21	"centered relative to the 1901-1907 mean..." this is repeating what is said on line 17. [Gareth S. Jones (Reviewer's comment ID #: 121-111)]	Caption revised.
9-1100	A	104:20	104:21	True, but this should be before, and qualified by, the sentence in lines 17-19. [Daithi Stone (Reviewer's comment ID #: 256-18)]	Caption revised.
9-1101	A	104:22	104:25	Omit after "Where not available...", this applies to another figure. [Daithi Stone (Reviewer's comment ID #: 256-19)]	Caption revised.
9-1102	A	104:24	104:24	What does this part mean? On line 16-17 it has already been stated that the observations	Caption revised.

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				have been made with respect to 1901-1997. This sounds like it is just repeating that. [Gareth S. Jones (Reviewer's comment ID #: 121-109)]	
9-1103	A	105:0	105:	Misleading, because there is no such "linear trend" from 1901to 2005, only a series of sequences, some rising, some falling for which a linear trend line is inappropriate [VINCENT GRAY (Reviewer's comment ID #: 88-1463)]	Rejected. Linear trend is an accepted way of presenting changes over time.
9-1104	A	105:0		Fig 9.4.2: Patterns of small-scale details are in some areas almost identical in simulated and observed pannels. See for example the pattern over Siberia in the left top two pannels. This seems improbable and inveracious. [Fons Baede (Reviewer's comment ID #: 9-25)]	Caption revised. Apparent similarity resulted from data masking. Details on masking provided in Appendix 9.C..
9-1105	A	105:0		Figure 9.4.2 What is grey shading? [Joanna Haigh (Reviewer's comment ID #: 95-14)]	See 9-1104.
9-1106	A	105:0		Figure 9.4.2: Suggest amending map plots to centre on greenwich meridian, i.e. showing 180W to 180E, that way none of the continents will be significantly split in two. (Unless this plot is pointing out something interesting in the pacific. [Gareth S. Jones (Reviewer's comment ID #: 121-112)]	Accepted. Maps now centered on longitude 0.
9-1107	A	105:0		Figure 9.4.2: It would be helpful to show the early 20th century trends also, revealing any differences with the later period trend. [Gareth S. Jones (Reviewer's comment ID #: 121-113)]	Rejected. Insufficient space.
9-1108	A	105:0		Figure 9.4.2. In the lower panels, the red obliterates the blue where they overlap. Change the colour or shading scheme to make the ranges of the ALL and NAT simulations more distinct. [David Parker (Reviewer's comment ID #: 195-144)]	Rejected. Red and blue domains can be distinguished.
9-1109	A	105:6	105:17	Figure 9.4.2: It looks like this graph has been created especially for this report. If so more details should be given about what processing was done. e.g. Do the top plots show trends only where data is available for the whole periods concerned? If so is this done on annual or monthly data? Should also note that the grey shading is representing missing data from the observations. [Gareth S. Jones (Reviewer's comment ID #: 121-114)]	Accepted. Details provided in electronic supplement. See Appendix 9.C.
9-1110	A	105:8	105:10	Omit "Where not available..." sentence, this does not apply here. [Daithi Stone (Reviewer's comment ID #: 256-20)]	Accepted. Caption revised.
9-1111	A	106:0	106:	As with the others, leaves out the most important influences, and ignores bias [VINCENT GRAY (Reviewer's comment ID #: 88-1464)]	Rejected. Reviewer provides no justification for this assertion.
9-1112	A	106:0	107:	Figs 9.4.3 and 4: Caption sentences regarding significance level should be clarified. [J. David Neelin (Reviewer's comment ID #: 187-18)]	Sentence revised.
9-1113	A	106:0		Figure 9.4.3. Use HadCRUT3 (Brohan et al., 2006) for the observations. [David Parker (Reviewer's comment ID #: 195-145)]	Accepted.

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9-1114	A	106:1	106:1	What does "Error!" appear here? [Michael Mann (Reviewer's comment ID #: 156-45)]	Corrected.
9-1115	A	106:4	106:10	Was the model data processed in the same way as the observations? Was observed data availability dealt with by the models? How were the significance ranges calculated? As this plot looks specially made for this report, much more information is needed. [Gareth S. Jones (Reviewer's comment ID #: 121-116)]	More detail now provided in supplementary information (Appendix 9.C).
9-1116	A	106:6	106:8	Why are not all available model data not used? In particular why is HadGEM1 anthropogenic +natural run not included in this plot, the data is available on the IPCC 4th Assessment Model Output database. [Gareth S. Jones (Reviewer's comment ID #: 121-115)]	Include all available models that meet climate drift constraint. See Appendix 9.C. HadGEM1 now included.
9-1117	A	107:0	107:	As with the others, leaves out the most important influences, and ignores bias [VINCENT GRAY (Reviewer's comment ID #: 88-1465)]	Rejected. See 9-1111.
9-1118	A	107:0		Figure 9.4.4 Do the confidence ranges given on the RHS correspond to all the regions? This seems rather odd, would expect different ranges for the different regions, because of different areas, and climate signals. [Gareth S. Jones (Reviewer's comment ID #: 121-117)]	Should be same in all regions because power is shown on a log scale. Use 5-95% intervals.
9-1119	A	107:0		Figure 9.4.4. Use HadCRUT3 (Brohan et al., 2006) for the observations. [David Parker (Reviewer's comment ID #: 195-146)]	Accepted.
9-1120	A	107:9	107:12	Whilst statistically most models might be consistent with the observed variability on scales 10 years and greater, the plots do seem to show a consistent greater variability on timescales upto ~50 years. Is this worth noting, as the reader will notice it for themselves and wonder why no mention is made. [Gareth S. Jones (Reviewer's comment ID #: 121-118)]	Rejected. Primary interest here is in time scales used for detection and attribution research.
9-1121	A	108:0	108:	Misleading, because there is no such " linear trend" from 1901 to 2005, and certainly not from 1950 to 1999 . [VINCENT GRAY (Reviewer's comment ID #: 88-1466)]	Rejected. Standard analysis.
9-1122	A	108:0		Figure 9.4.5. Line 8 of the caption refers to light blue bars but none can be seen. [David Parker (Reviewer's comment ID #: 195-147)]	Revised caption to delete "light blue"
9-1123	A	108:0		It may be worth adding data from Crooks, Allen, Stott (2006) and/or Stone, Allen, Stott (2006) to this figure. [Daithi Stone (Reviewer's comment ID #: 256-21)]	Rejected. Adding these papers will not improve balance of information in chapter.
9-1124	A	108:9	108:12	Recommend not using the model ID numbers here but rather the model names. [Gareth S. Jones (Reviewer's comment ID #: 121-119)]	Accepted.
9-1125	A	109:0	109:	Again, ignores important climate influenecs such as El Niño and surface human influences such as urbanisation, energy emissions and Land-use chnges Linear trends per decade are just as ridiculous a lineartrends per century	Rejected. Reviewer provides no justification for the assertion that these factors are needed to explain the

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				[VINCENT GRAY (Reviewer's comment ID #: 88-1467)]	warming.
9-1126	A	109:8	109:10	What is the selection criteria for including and not including available AR4 models. Have any models been excluded that may have shown a better agreement in the early part of the century with observations? [Gareth S. Jones (Reviewer's comment ID #: 121-120)]	Rejected. Diagram is from published study.
9-1127	A	109:12	109:13	I like the idea of showing the persistence hindcast, but is it supposed to show persistence from the previous decades observed anomalies? If so it doesn't seem to always match the previous decade's observed anomaly, e.g. 1990 persistence lower than 1980 observed, same for 1980 and 1970. Is this a mistake, or is the plot showing something else? [Gareth S. Jones (Reviewer's comment ID #: 121-121)]	Well spotted. The persistence hindcast infact shows the 5-95% range of the posterior distribution that is conditioned on persisted anomalies – and this tends to slightly deflate the anomalies. The relevant sentence in the caption has been slightly revised.
9-1128	A	110:0	110:	Linear trends per decade are just as ridiculous a lineartrends per century [VINCENT GRAY (Reviewer's comment ID #: 88-1468)]	Don't know what reviewer wants here.
9-1129	A	111:0	111:	This map is grossly misleading, as none of the "observed" graphs have been properly corrected for urban and instrumental effect. Most cannot, because the necessary number of stations for comparison are not available. However, you do not even have the sense to provide the corrected records for the contiguous zUnited Staes and for China, which have a very different appearance from the ones you exhibt, indicating the fact that all of them are faulty [VINCENT GRAY (Reviewer's comment ID #: 88-1469)]	Rejected. There is considerable evidence that the observed warming is not due to urban and instrumental effects.
9-1130	A	111:0		Figure 9.4.8: This is a very good diagram. It would be great if the diagram was made full page size to improve the readability of the regions. Another comment is that the current map projection can put too much weight on the reconstructions of the high latitudes. Not sure if there is a better way of showing this, maybe showing each box in size proportion to the area covered or amount of data going into it? [Gareth S. Jones (Reviewer's comment ID #: 121-122)]	Partially accepted. Layout will be determined by the TSU.
9-1131	A	111:0		Figure 9.4.8. Use HadCRUT3 (Brohan et al., 2006) for the observations and make the axis-labels on the inset graphs bigger. [David Parker (Reviewer's comment ID #: 195-148)]	Accepted.
9-1132	A	111:1		Figure 9.4.8. Given the enormous significance that this figure is likely to have, I am a bit concerned that no errors are shown for the black lines representing observed regional temperatures. [Martin Manning (Reviewer's comment ID #: 155-68)]	Partly rejected. Would make figure too fussy to be read. Would mix different sorts of uncertainty (model versus data; sampling error versus instrumental errors). Revised figure gives an indication where coverage may be inadequate to represent a region.

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					Details are provided in the supplementary information (Appendix 9.C).
9-1133	A	111:6		"C20C3M" to "20C3M" [Daithi Stone (Reviewer's comment ID #: 256-22)]	Accepted.
9-1134	A	111:7		The naturally forced simulations used here were not in the IPCC AR4 20C3M database. Does this reference refer to that database? [Daithi Stone (Reviewer's comment ID #: 256-23)]	Caption revised.
9-1135	A	111:16	111:18	The naturally forced simulations are centred relative to the 1901-1997 mean of the corresponding all forced simulation in the region of interest. [Daithi Stone (Reviewer's comment ID #: 256-24)]	Accepted. Caption revised.
9-1136	A	113:0	113:	As usual, you leave out El Niño which had a very large effect in the troposphere in 1999 and may be continuing [VINCENT GRAY (Reviewer's comment ID #: 88-1470)]	Noted. Relevance?
9-1137	A	113:0		In the figure legend and caption, there is a reference to "ERA". It should be specified whether this refers to ERA-15 (likely because of the date range quoted) or ERA-40. [Adrian Simmons (Reviewer's comment ID #: 242-132)]	Accepted. Caption revised.
9-1138	A	114:0	114:	The "signal strength" is falsely pretended to be linear when it is actually periodic. The models are linear, the "signal" wobbles back and forth, so the models are wrong [VINCENT GRAY (Reviewer's comment ID #: 88-1471)]	Rejected. This is a very simplistic idea of "signal"
9-1139	A	114:18	114:18	Replace "four PCM simulations" with "four PCM simulations forced by observed solar and volcanic variability" (see Barnett et al. 2005) [Govt. of Finland (Reviewer's comment ID #: 2009-115)]	Accepted. Caption revised.
9-1140	A	115:0	115:	Again, we are talking of "decadal means" as if there was such a thing. The pressure varies in a non linear fashion [VINCENT GRAY (Reviewer's comment ID #: 88-1472)]	Rejected. Reviewer provides no justification for this assertion.
9-1141	A	117:0	117:	Very poor agreement [VINCENT GRAY (Reviewer's comment ID #: 88-1473)]	Don't know what reviewer wants here?
9-1142	A	119:0	119:	The climate is never in equilibrium. [VINCENT GRAY (Reviewer's comment ID #: 88-1474)]	Don't know what reviewer is getting at with this comment.
9-1143	A	119:0		Fig 9.6.1 Including the results of say Forest et al 2002,2006 (with expert prior), Annan and HargreavesGRL 2006, Hegerl et al 2006 would help to give a more credible impression. Perhaps a new panel would help. [James Annan (Reviewer's comment ID #: 6-12)]	Rejected. These points are made in text.
9-1144	A	119:0		Fig. 9.6.1: I think one cannot show Annan et al 2005 as a serious estimate here, as neither all glacial forcings nor any changes in ocean heat transport were allowed for. The result is	Rejected. We feel we cannot ignore the Annan result. Limitations are discussed

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				strongly affected by these omissions of first-order effects and thus is not robust on the basis of current scientific knowledge. Also, how can you show a dotted line between 3-4ºC for Annan et al, if none of the models in their ensemble had a climate sensitivity below 4ºC? How do you know it doesn't start at 2.7 ºC? Is this just making up non-existing data, in Jan Hendrik Schön style, or is there data for that? The caption "Note that the latter range is extended by dots towards lower sensitivities than sampled by their model" suggests the former - this is not worthy of IPCC. This strange manoevring with dots (or an arrow, in the corresponding chapter 10 figure) just tries to gloss over the methodological flaws of this study - it simply should not be cited, rather than compromising the scientific integrity and credibility of such an important figure of the AR4. [Stefan Rahmstorf (Reviewer's comment ID #: 206-31)]	in the text. Caption notes that the model used by Annan as structural limitations. Visual presentation of the Annan result slightly revised. See also 9-1146.
9-1145	A	119:0		figure # 9.6.1 Not sure if Schneider LGM06 has a PDF associated with it but if it has, then it has been omitted from the top part of the plot. [David Sexton (Reviewer's comment ID #: 233-14)]	Rejected. Chose not to show any pdfs from LGM estimates.
9-1146	A	119:5	119:14	The disagreement between Annan et al. and Schneider von Deimling et al. is not only due use of different models but also due to more realistic boundary conditions in the study by Schneider von Deimling et al.: in their study, LGM-specific dust & vegetation forcing is accounted for what is not the case in Annan et al. (as it is not in the PMIP-2 protocol). Schneider von Deimling et al. also estimated the magnitude of this effect in their publication: proper accounting for both boundary conditions shifts inferred climate sensitivity down by a value up to 1.4°C. Hence, the values derived by Annan et al. should be lowered by up to 1.4°C. Also, it should be noted that the values by Schneider von Deimling et al. are the unification of ranges derived from results for two different regions: tropical SST & East Antarctica. [Hermann Held (Reviewer's comment ID #: 104-12)]	Accepted. Figure revised to indicate possible shift downward of Annan et al. It is noted in text how pdfs are arrived at, suggestion to add this to figure rejected.
9-1147	A	120:0	120:	There is no linear "warming rate" over the "century" and you would get a different "linaer rate" wherever you chose to start or finish. Your surface record starts at 1856, Why not 1856-2005?The "warming" is highly irregular" and you can get any "rate you want depending where you start and where you finish [VINCENT GRAY (Reviewer's comment ID #: 88-1475)]	Rejected. Any reasonable choice of start and end dates leads you to a similar warming.
9-1148	A	120:5	120:6	The values for the individual models suggest that this is simply the warming at the doubling of CO2 (i.e., 70 years in the models), not the warming rate over 100 years. [Govt. of Finland (Reviewer's comment ID #: 2009-116)]	Agreed. Correct caption.
9-1149	A	121:0		Question 9.1, Figure 1. This is a good diagram, but it shows that the temperature distribution is positively skew, not Gaussian, so that the normalised anomaly for 2003 is a slight overstatement of how extreme the European summer of 2003 was.	Rejected. We do not associate a p-value with the normalised anomaly. .Also, this is reproduction of figure from a

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				[David Parker (Reviewer's comment ID #: 195-149)]	published paper.
9-1150	A	121:5		Suggest improving figure caption for readability by non-experts: Occurrence frequency distribution of Swiss seasonal summer temperatures for the period 1864-2003. The solid line (green) is the average occurrence frequency as fitted to a Gaussian distribution. A vertical line is shown for each year in the 137-yr record. The years 1909, 1947, and 2003 are labeled because they represent extreme years in the record. The values in the lower left corner.....' [David Wratt & David Fahey (Reviewer's comment ID #: 67-67)]	Partly agreed. Two sentences starting "A vertical…" and "The years…" have been added to the caption.
9-1151	A	122:0	122:	A completely phony graph. Ther are well-authenticated corrected mean annual surface records for the continental United States, and for China which you refuse to make use of. None of these supposed "observed" records have ever been published anywhere, and it is obvious from the effects of comprfehensive correction in USA and China, that all of the others would lose most of their "warming" if a proper correction procedure was available [VINCENT GRAY (Reviewer's comment ID #: 88-1476)]	Rejected. Indeed many of the observed records have been published. Much evidence that the warming is real and not due to instrumental changes.
9-1152	A	122:0		Question 9.1, Figure 1. Use HadCRUT3 (Brohan et al., 2006) for the observations. Tidy up caption from line 8 onwards to: "Of these 13 models, 11 are described in Chapter 8, Table 8.2.1 and are identified as follows by their Model ID given in that table: CCSM3 ... [omitting GFDL R30 and omitting 6:, 7: etc] ...HadCM3 (22,4). An additional two.....(Delworth et al., 2002). The blue band... forcings. Model simulations....HadCM3 (22,4). Each simulation....interest." [David Parker (Reviewer's comment ID #: 195-150)]	Accepted. Caption improved. Now use HadCRUT3.
9-1153	A	122:1	122:22	why are ocean regions not considered as regional panels? I would highly encourage the inclusion of a panel for the tropical North Atlantic, given its prominence in discussions of Atlantic tropical cyclone genesis. [Michael Mann (Reviewer's comment ID #: 156-46)]	Rejected. No literature as yet published. Note however that similar plots (produced identically) are displayed for some ocean regions in Ch 11. Reference to the presence of these figures is made in the text.
9-1154	A	122:3	122:22	The Polar regions are an obvious gap in the areas for which modelled and measured 20th C temperature anomalies are shown. We realise there is a problem with limited observational data for this region. However, leaving Antarctica (where we think the measured temperature trends are small?) out means this FAQ is likely to be heavily criticised by some for "cherry-picking" by leaving out the region with smallest trends. There might also be criticisms for leaving out the Arctic where we think both measurements and models give fairly large changes ? We suggest the lead authors for this chapter give serious consideration of including plots for Antarctica (and possibly the Arctic) if there is enough data. [David Wratt & David Fahey (Reviewer's comment ID #: 67-117)]	Rejected. Antarctica data too sparse.

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9-1155	A	122:4		This figure caption is much too complicated to be absorbed by the non-expert reader. Suggest revising to provide a simple explanation of 'what' the figure shows of relevance to the question. The model acronyms are best completely avoided and referenced if needed through the main chapter figure. [David Wratt & David Fahey (Reviewer's comment ID #: 67-70)]	Accepted. Caption simplified and cross-referenced to caption of figure 9.4.8 (which in turn points to supplementary information in Appendix A.9).
9-1156	A	122:11	122:15	Why are not all available model data not used? In particular why is HadGEM1 anthropogenic +natural run not included in this plot, the data is available on the IPCC 4th Assessment Model Output database. [Gareth S. Jones (Reviewer's comment ID #: 121-123)]	See 9-1116.
9-1157	A	122:16	122:18	See comment 24. [Daithi Stone (Reviewer's comment ID #: 256-25)]	Noted. Figure revised.